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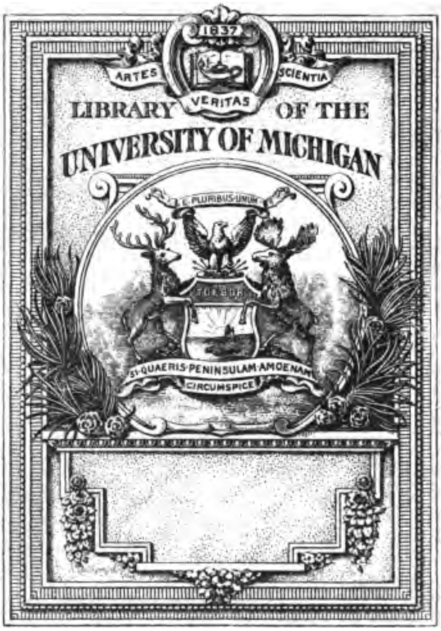
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A

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EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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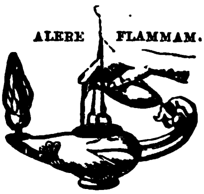
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Abetti.
Anderson.
Backhouse.
Barnard.
Campbell.
Chandler.
Christie.
Claxton.
Clerke, Miss.
Copeland.
Cowell.
Crommelin.
Denning.
Deslandres.
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No. 300. 1901. Vol. XXIV.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of the disk of Mercury is from the American 'Nautical Almanac,' and that of Jupiter V. from the 'Connaissance de Temps,' 1901.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon, except in the Ephemerides of Variable Stars, where Greenwich Civil Time is used, the day being reckoned from midnight to midnight.

Date.	Day of Year.	Fr. of Year.	Sun		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 6	6	·014	4 4	20 7	22 33 S	0 5 52	19 1 29	○ 4 12 14
13	13	·033	4 14	20 3	21 33	8 45	19 29 5	☾ 12 8 38
20	20	·052	4 25	19 57	20 12	11 8	19 56 41	● 20 2 36
27	27	·071	4 37	19 48	18 33	12 54	20 24 17	☽ 26 21 52
Feb. 3	34	·090	4 50	19 37	16 38	14 0	20 51 52	○ 3 3 30
10	41	·110	5 3	19 25	14 29	14 26	21 19 28	☾ 11 6 12
17	48	·129	5 16	19 14	12 7	14 15	21 47 4	● 18 14 45
24	55	·148	5 29	18 57	9 36	13 30	22 14 40	☽ 25 6 38
Mar. 3	62	·167	5 41	18 43	6 58	12 14	22 42 16	○ 4 20 4
10	69	·186	5 53	18 27	4 16	10 34	23 9 52	☾ 13 1 6
17	76	·205	6 5	18 11	1 30 S	8 39	23 37 28	● 20 0 53
24	83	·225	6 17	17 55	1 16 N	6 34	0 5 4	☽ 26 16 39
31	90	·244	6 29	17 39	4 0	4 25	0 32 39	
April 7	97	·263	6 40	17 23	6 40	2 20	1 0 15	○ 3 13 20
14	104	·282	6 52	17 8	9 15	0 0 25	1 27 51	☾ 11 15 57
21	111	·301	7 4	16 53	11 43	23 58 48	1 55 27	● 18 9 37
28	118	·320	7 15	16 39	14 1	57 30	2 23 3	☽ 25 4 15
May 5	125	·340	7 27	16 26	16 8	56 38	2 50 39	○ 3 6 19
12	132	·359	7 38	16 14	18 2	56 12	3 18 15	☾ 11 2 38
19	139	·378	7 48	16 4	19 41	56 15	3 45 51	● 17 17 38
26	146	·397	7 57	15 56	21 3	56 46	4 13 26	☽ 24 17 40
June 2	153	·416	8 5	15 50	22 8	57 39	4 41 2	○ 1 21 53
9	160	·435	8 12	15 46	22 54	23 58 51	5 8 38	☾ 9 10 0
16	167	·455	8 16	15 44	23 20	0 0 17	5 36 14	● 16 1 33
23	174	·474	8 19	15 45	23 27	1 48	6 3 50	☽ 23 8 59
30	181	·493	8 18	15 49	23 13	3 16	6 31 26	
July 7	188	·512	8 15	15 54	22 39	4 32	6 59 2	○ 1 11 18
14	195	·531	8 10	16 1	21 46	5 32	7 26 38	☾ 8 15 20
21	202	·550	8 3	16 10	20 34	6 8	7 54 14	● 15 10 10
28	209	·570	7 53	16 20	19 6	6 17	8 21 49	☽ 23 1 58
								○ 30 22 34
Aug. 4	216	·580	7 42	16 30	17 22	5 55	8 49 25	☾ 6 20 2
11	223	·608	7 30	16 41	15 25	5 6	9 17 1	● 13 20 27
18	230	·627	7 16	16 52	13 15	3 49	9 44 37	☽ 21 19 52
25	237	·646	7 2	17 3	10 55	2 6	10 12 13	○ 29 8 21
Sept. 1	244	·665	6 46	17 14	8 27	0 0 4	10 39 49	☾ 5 1 27
8	251	·685	6 31	17 25	5 52	23 57 46	11 7 25	● 12 9 18
15	258	·704	6 15	17 37	3 12	55 21	11 35 1	☽ 20 13 33
22	265	·723	5 59	17 48	0 29 N	52 53	12 2 36	○ 27 17 36
29	272	·742	5 42	17 59	2 14 S	50 29	12 30 12	
Oct. 6	279	·761	5 26	18 11	4 57	48 17	12 57 48	☾ 4 8 52
13	286	·780	5 11	18 23	7 37	46 24	13 25 24	● 12 1 11
20	293	·800	4 56	18 35	10 12	44 57	13 53 0	☽ 20 5 58
27	300	·819	4 42	18 47	12 39	43 59	14 20 36	○ 27 3 6
Nov. 3	307	·838	4 29	19 0	14 56	43 38	14 48 12	☾ 2 19 24
10	314	·857	4 17	19 12	17 2	43 58	15 15 48	● 10 19 34
17	321	·876	4 7	19 25	18 54	44 59	15 43 23	☽ 18 20 23
24	328	·895	3 58	19 36	20 29	46 39	16 10 59	○ 25 13 18
Dec. 1	335	·915	3 52	19 46	21 45	48 58	16 38 35	☾ 2 9 50
8	342	·934	3 49	19 56	22 41	41 49	17 6 11	● 10 14 53
15	349	·953	3 48	20 3	23 15	55 3	17 33 47	☽ 18 8 35
22	356	·972	3 50	20 7	23 27	23 58 30	18 1 23	○ 25 0 16
29	363	·991	3 55	20 9	23 16 S	0 1 58	18 28 59	

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	17 23	9 11	20 33 N	−48·7 M.
2	W	18 26	10 9	21 25	−60·8 M.
3	Th	19 16	11 7	20 57	−73·0 M.
4	F	rises	12 3	19 15	−85·2 M.
5	S	5 9	12 56	16 31	+82·7 E.
6	**S**	6 21	13 46	13 1	+70·6 E.
7	M	7 32	14 32	9 0	+58·4 E.
8	Tu	8 40	15 17	4 41	+46·2 E.
9	W	9 46	15 59	0 16 N	+34·1 E.
10	Th	10 52	16 41	4 5 S	+21·9 E.
11	F	11 58	17 23	8 14	+ 9·7 E.
12	S	13 4	18 7	12 3	− 2·4 E.
13	**S**	14 8	18 51	15 24	−14·6 E.
14	M	15 10	19 38	18 10	−26·7 E.
15	Tu	16 13	20 28	20 9	−38·9 E.
16	W	17 12	21 30	21 13	−51·0 E.
17	Th	18 2	22 13	21 14	−63·2 E.
18	F	18 47	23 7	20 6	−75·3 E.
19	S	sets	......	17 49	−87·5 E.
20	**S**	4 47	0 2	14 28	+80·3 M.
21	M	6 5	0 55	10 15	+68·2 M.
22	Tu	7 25	1 48	5 25	+56·0 M.
23	W	8 45	2 39	0 16 S	+43·8 M.
24	Th	10 7	3 31	4 54 N	+31·7 M.
25	F	11 25	4 22	9 47	+19·5 M.
26	S	12 44	5 15	14 5	+ 7·4 M.
27	**S**	14 1	6 10	17 33	− 4·8 M.
28	M	15 12	7 5	19 58	−17·0 M.
29	Tu	16 15	8 2	21 10	−29·1 M.
30	W	17 10	8 59	21 6	−41·3 M.
31	Th	17 54	9 54	19 51 N	−53·4 M.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	18 29	10 47	17 31 N	−65·6 M.
2	S	18 59	11 37	14 20	−77·7 M.
3	**S**	rises	12 25	10 32	−89·9 M.
4	M	6 23	13 10	6 21	+77·9 E.
5	Tu	7 30	13 54	1 58 N	+65·8 E.
6	W	8 39	14 36	2 25 S	+53·6 E.
7	Th	9 43	15 19	6 39	+41·4 E.
8	F	10 47	16 1	10 35	+29·3 E.
9	S	11 51	16 45	14 6	+17·1 E.
10	**S**	12 56	17 31	17 2	+ 4·9 E.
11	M	13 58	18 18	19 18	− 7·2 E.
12	Tu	14 55	19 8	20 43	−19·4 E.
13	W	15 50	20 0	21 9	−31·6 E.
14	Th	16 37	20 53	20 32	−43·7 E.
15	F	17 17	21 47	18 47	−55·9 E.
16	S	17 53	22 41	15 55	−68·1 E.
17	**S**	18 22	23 35	12 4	−80·2 E.
18	M	sets	......	7 26	+87·6 M.
19	Tu	6 21	0 28	2 18 S	+75·4 M.
20	W	7 44	1 21	3 1 N	+63·3 M.
21	Th	9 6	2 14	8 9	+51·1 M.
22	F	10 28	3 9	12 46	+38·9 M.
23	S	11 47	4 4	16 32	+26·8 M.
24	**S**	13 2	5 1	19 15	+14·6 M.
25	M	14 9	5 58	20 45	+ 2·4 M.
26	Tu	15 6	6 55	20 59	− 9·8 M.
27	W	15 53	7 50	20 2	−22·0 M.
28	Th	16 30	8 43	18 2 N	−34·1 M.

MARCH.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	17 1	9 33	15 9 N	−46·3 M.
2	S	17 26	10 21	11 36	−58·5 M.
3	**S**	17 48	11 6	7 36	−70·7 M.
4	M	18 11	11 50	3 20 N	−82·8 M.
5	Tu	rises	12 33	1 0 S	+85·0 E.
6	W	7 31	13 15	5 15	+72·8 E.
7	Th	8 35	13 58	9 16	+60·6 E.
8	F	9 40	14 41	12 53	+48·5 E.
9	S	10 43	15 26	15 59	+36·3 E.
10	**S**	11 45	16 12	18 25	+24·1 E.
11	M	12 44	17 0	20 5	+11·9 E.
12	Tu	13 38	17 50	20 51	− 0·3 E.
13	W	14 27	18 41	20 38	−12·5 E.
14	Th	15 10	19 33	19 21	−24·6 E.
15	F	15 46	20 26	17 2	−36·8 E.
16	S	16 19	21 19	13 42	−49·0 E.
17	**S**	16 47	22 12	9 29	−61·2 E.
18	M	17 13	23 5	4 36 S	−73·4 E.
19	Tu	17 37	23 59	0 39 N	−85·5 E.
20	W	sets	......	5 57	+82·3 M.
21	Th	8 0	0 55	10 54	+70·1 M.
22	F	9 25	1 52	15 7	+57·9 M.
23	S	10 44	2 50	18 17	+45·7 M.
24	**S**	11 57	3 49	20 12	+33·6 M.
25	M	13 0	4 48	20 48	+21·4 M.
26	Tu	13 50	5 45	20 8	+ 9·2 M.
27	W	14 31	6 39	18 21	− 3·0 M.
28	Th	15 4	7 31	15 41	−15·2 M.
29	F	15 31	8 19	12 19	−27·4 M.
30	S	15 55	9 5	8 28	−39·5 M.
31	**S**	16 16	9 49	4 20	−51·7 M.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	16 36	10 31	0 5 N	−64·0 M.
2	Tu	16 55	11 13	4 9 S	−76·2 M.
3	W	rises	11 56	8 11	−88·4 M.
4	Th	7 30	12 39	11 53	+79·4 E.
5	F	8 34	13 23	15 6	+67·2 E.
6	S	9 36	14 9	17 42	+55·0 E.
7	**S**	10 36	14 56	19 33	+42·9 E.
8	M	11 32	15 45	20 33	+30·7 E.
9	Tu	12 22	16 35	20 36	+18·5 E.
10	W	13 6	17 25	19 40	+ 6·3 E.
11	Th	13 43	18 16	17 43	− 5·9 E.
12	F	14 16	19 7	14 50	−18·1 E.
13	S	14 44	19 58	11 5	−30·3 E.
14	**S**	15 11	20 50	6 36	−42·4 E.
15	M	15 35	21 42	1 37 S	−54·6 E.
16	Tu	16 1	22 36	3 35 N	−66·8 E.
17	W	16 30	23 33	8 41	−79·0 E.
18	Th	sets	......	13 17	+88·7 M.
19	F	8 15	0 31	16 59	+76·5 M.
20	S	9 35	1 32	19 29	+64·3 M.
21	**S**	10 44	2 33	20 36	+52·2 M.
22	M	11 42	3 33	20 20	+40·0 M.
23	Tu	12 28	4 31	18 51	+27·8 M.
24	W	13 5	5 25	16 21	+15·6 M.
25	Th	13 34	6 15	13 6	+ 3·3 M.
26	F	14 0	7 3	9 20	− 8·9 M.
27	S	14 21	7 47	5 15	−21·1 M.
28	**S**	14 41	8 30	1 2 N	−33·3 M.
29	M	15 1	9 12	3 11 S	−45·4 M.
30	Tu	15 22	9 54	7 14 S	−57·6 M.

MAY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	15 44	10 37	11 0 S	−69·9 M.
2	Th	16 9	11 21	14 20	−82·1 M.
3	F	rises	12 6	17 6	+85·7 E.
4	S	8 29	12 53	19 8	+73·5 E.
5	**S**	9 26	13 42	20 20	+61·3 E.
6	M	10 20	14 32	20 37	+49·0 E.
7	Tu	11 5	15 22	19 55	+36·8 E.
8	W	11 43	16 12	18 14	+24·6 E.
9	Th	12 17	17 2	15 38	+12·4 E.
10	F	12 45	17 51	12 11	+ 0·2 E.
11	S	13 12	18 41	8 3	−12·0 E.
12	**S**	13 37	19 31	3 23 S	−24·2 E.
13	M	14 0	20 22	1 36 N	−36·4 E.
14	Tu	14 27	21 16	6 38	−48·7 E.
15	W	14 56	22 12	11 22	−60·9 E.
16	Th	15 31	23 11	15 27	−73·2 E.
17	F	sets	……	18 30	−85·4 E.
18	S	8 22	0 13	20 16	+82·5 M.
19	**S**	9 27	1 15	20 35	+70·3 M.
20	M	10 20	2 15	19 33	+58·1 M.
21	Tu	11 1	3 13	17 20	+45·9 M.
22	W	11 35	4 7	14 14	+33·6 M.
23	Th	12 2	4 57	10 31	+21·4 M.
24	F	12 26	5 43	6 26	+ 9·2 M.
25	S	12 48	6 28	2 12 N	− 3·0 M.
26	**S**	13 7	7 10	2 4 S	−15·2 M.
27	M	13 27	7 52	6 11	−27·4 M.
28	Tu	13 49	8 35	10 2	−39·6 M.
29	W	14 12	9 18	13 30	−51·9 M.
30	Th	14 41	10 3	16 26	−64·1 M.
31	F	15 13	10 50	18 41 S	−76·3 M.

JUNE.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	7 21	11 38	20 8 S	−88·5 M.
2	**S**	8 15	12 28	20 39	+79·2 E.
3	M	9 4	13 18	20 12	+67·0 E.
4	Tu	9 45	14 9	18 45	+54·8 E.
5	W	10 20	14 59	16 21	+42·6 E.
6	Th	10 51	15 49	13 6	+30·4 E.
7	F	11 17	16 38	9 9	+18·1 E.
8	S	11 42	17 26	4 40 S	+ 5·9 E.
9	**S**	12 5	18 16	0 8 N	− 6·3 E.
10	M	12 29	19 7	5 1	−18·5 E.
11	Tu	12 56	20 0	9 45	−30·7 E.
12	W	13 26	20 56	13 59	−43·0 E.
13	Th	14 4	21 55	17 23	−55·2 E.
14	F	14 50	22 56	19 41	−67·4 E.
15	S	15 45	23 57	20 38	−79·6 E.
16	**S**	sets	……	20 11	+88·1 M.
17	M	8 54	0 57	18 26	+75·9 M.
18	Tu	9 32	1 53	15 38	+63·7 M.
19	W	10 3	2 46	12 4	+51·5 M.
20	Th	10 29	3 35	8 1	+39·3 M.
21	F	10 52	4 22	3 43 N	+27·1 M.
22	S	11 12	5 6	0 37 S	+14·9 M.
23	**S**	11 32	5 49	4 51	+ 2·7 M.
24	M	11 55	6 31	8 49	− 9·6 M.
25	Tu	12 16	7 15	12 26	−21·8 M.
26	W	12 43	7 59	15 33	−34·0 M.
27	Th	13 14	8 45	18 2	−46·2 M.
28	F	13 51	9 32	19 45	−58·4 M.
29	S	14 34	10 22	20 35	−70·7 M.
30	**S**	15 28	11 13	20 27 S	−82·9 M.

JULY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	7 44	12 4	19 17 S	+84·8 E.
2	Tu	8 21	12 55	17 8	+72·6 E.
3	W	8 55	13 46	14 4	+60·4 E.
4	Th	9 23	14 36	10 15	+48·1 E.
5	F	9 48	15 25	5 52	+35·9 E.
6	S	10 12	16 14	1 7	+23·7 E.
7	**S**	10 35	17 4	3 44 N	+11·5 E.
8	M	11 0	17 55	8 27	− 0·7 E.
9	Tu	11 30	18 48	12 45	−12·9 E.
10	W	12 2	19 44	16 21	−25·1 E.
11	Th	12 43	20 43	18 58	−37·3 E.
12	F	13 32	21 42	20 23	−49·6 E.
13	S	14 33	22 42	20 29	−61·8 E.
14	**S**	15 41	23 39	19 16	−74·1 E.
15	M	sets	……	16 55	−86·3 E.
16	Tu	8 3	0 34	13 38	+81·5 M.
17	W	8 30	1 25	9 45	+69·3 M.
18	Th	8 54	2 13	5 29	+57·1 M.
19	F	9 16	2 59	1 6 N	+44·9 M.
20	S	9 36	3 43	3 14 S	+32·6 M.
21	**S**	9 58	4 27	7 21	+20·4 M.
22	M	10 20	5 10	11 7	+ 8·2 M.
23	Tu	10 45	5 54	14 24	− 4·1 M.
24	W	11 14	6 39	17 7	−16·3 M.
25	Th	11 49	7 25	19 7	−28·5 M.
26	F	12 29	8 14	20 16	−40·7 M.
27	S	13 18	9 4	20 30	−53·0 M.
28	**S**	14 15	9 55	19 43	−65·2 M.
29	M	15 21	10 47	17 55	−77·4 M.
30	Tu	16 31	11 38	15 9	−89·6 M.

AUGUST.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	7 52	13 20	7 14 S	+66·0 E.
2	F	8 18	14 10	2 31	+53·8 E.
3	S	8 41	15 1	2 24 N	+41·6 E.
4	**S**	9 8	15 52	7 12	+29·4 E.
5	M	9 35	16 45	11 32	+17·1 E.
6	Tu	10 5	17 40	15 27	+ 4·9 E.
7	W	10 43	18 37	18 13	− 7·4 E.
8	Th	11 30	19 34	19 54	−19·6 E.
9	F	12 24	20 32	20 27	−31·8 E.
10	S	13 25	21 29	19 40	−44·0 E.
11	**S**	14 35	22 24	17 44	−56·2 E.
12	M	15 48	23 16	14 50	−68·4 E.
13	Tu	17 0	……	11 13	−80·6 E.
14	W	sets	0 5	7 7	+87·1 M.
15	Th	7 20	0 52	2 47 N	+74·9 M.
16	F	7 42	1 37	1 34 S	+62·7 M.
17	S	8 2	2 21	5 46	+50·5 M.
18	**S**	8 25	3 4	9 41	+38·3 M.
19	M	8 48	3 48	13 8	+26·1 M.
20	Tu	9 17	4 33	16 3	+13·8 M.
21	W	9 48	5 19	18 17	+ 1·6 M.
22	Th	10 25	6 6	19 44	−10·6 M.
23	F	11 9	6 55	20 19	−22·8 M.
24	S	12 2	7 45	19 56	−35·0 M.
25	**S**	13 4	8 36	18 34	−47·2 M.
26	M	14 10	9 27	16 12	−59·4 M.
27	Tu	15 23	10 19	12 56	−71·6 E.
28	W	16 39	11 10	8 53	−83·8 M.
29	Th	rises	12 1	4 15 S	+84·0 E.
30	F	6 43	12 53	0 41 N	+71·8 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	7 38	14 40	10 17 N	+47·3 E.
2	M	8 9	15 35	14 18	+35·1 E.
3	Tu	8 46	16 32	17 25	+22·9 E.
4	W	9 29	17 30	19 26	+10·7 E.
5	Th	10 21	18 28	20 12	− 1·5 E.
6	F	11 20	19 24	19 44	−13·7 E.
7	S	12 26	20 19	18 8	−25·9 E.
8	**S**	13 37	21 11	15 33	−38·1 E.
9	M	14 47	22 0	12 13	−50·3 E.
10	Tu	15 57	22 47	8 20	−62·6 E.
11	W	17 6	23 32	4 9 N	−74·8 E.
12	Th	18 14	……	0 9 S	−87·0 E.
13	F	sets	0 16	4 22	+80·9 M.
14	S	6 29	1 0	8 21	+68·6 M.
15	**S**	6 52	1 44	11 56	+56·4 M.
16	M	7 19	2 28	15 1	+44·2 M.
17	Tu	7 48	3 13	17 27	+32·0 M.
18	W	8 24	4 0	19 8	+19·8 M.
19	Th	9 4	4 47	19 59	+ 7·6 M.
20	F	9 52	5 36	19 57	− 4·6 M.
21	S	10 50	6 26	18 58	−16·8 M.
22	**S**	11 52	7 16	17 2	−29·0 M.
23	M	13 1	8 6	14 12	−41·2 M.
24	Tu	14 14	8 57	10 32	−53·4 M.
25	W	15 30	9 48	6 11	−65·6 M.
26	Th	16 49	10 39	1 23 S	−77·7 M.
27	F	18 11	11 32	3 38 N	−89·9 M.
28	S	rises	12 27	8 30	+77·9 E.
29	**S**	6 7	13 24	12 52	+65·7 E.
30	M	6 43	14 22	16 23 N	+53·5 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	7 25	15 22	18 48 N	+41·3 E.
2	W	8 17	16 21	19 56	+29·1 E.
3	Th	9 15	17 20	19 46	+16·9 E.
4	F	10 21	18 15	18 24	+ 4·7 E.
5	S	11 29	19 8	16 2	− 7·5 E.
6	**S**	12 40	19 57	12 53	−19·7 E.
7	M	13 50	20 45	9 11	−31·8 E.
8	Tu	14 56	21 30	5 8	−44·0 E.
9	W	16 3	22 14	0 56 N	−56·2 E.
10	Th	17 10	22 57	3 15 S	−68·4 E.
11	F	18 16	23 40	7 15	−80·6 E.
12	S	sets	……	10 55	+87·2 M.
13	**S**	5 22	0 24	14 8	+75·0 M.
14	M	5 51	1 9	16 44	+62·8 M.
15	Tu	6 25	1 55	18 37	+50·6 M.
16	W	7 3	2 43	19 42	+38·4 M.
17	Th	7 48	3 31	19 55	+26·2 M.
18	F	8 41	4 19	19 14	+14·0 M.
19	S	9 39	5 8	17 38	+ 1·9 N.
20	**S**	10 44	5 57	15 10	−10·3 M.
21	M	11 53	6 46	11 55	−22·5 M.
22	Tu	13 5	7 35	7 57	−34·7 M.
23	W	14 19	8 25	3 26 S	−46·9 M.
24	Th	15 39	9 16	1 25 N	−59·0 M.
25	F	16 59	10 9	6 19	−71·2 M.
26	S	18 23	11 5	10 57	−83·3 M.
27	**S**	rises	12 4	14 56	+84·5 E.
28	M	5 17	13 5	17 54	+72·3 E.
29	Tu	6 4	14 7	19 35	+60·1 E.
30	W	7 1	15 8	19 52	+47·9 E.
31	Th	8 8	16 7	18 50 N	+35·7 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	9 18	17 2	16 41 N	+23·6 E.
2	S	10 28	17 54	13 40	+11·4 E.
3	**S**	11 41	18 43	10 2	− 0·7 E.
4	M	12 49	19 28	6 3	−12·9 E.
5	Tu	13 56	20 13	1 53 N	−25·1 E.
6	W	15 2	20 56	2 17 S	−37·3 E.
7	Th	16 6	21 39	6 19	−49·5 E.
8	F	17 10	22 22	10 3	−61·7 E.
9	S	18 13	23 7	13 22	−73·8 E.
10	**S**	19 16	23 53	16 7	−86·0 E.
11	M	sets	……	18 12	+81·9 M.
12	Tu	5 1	0 39	19 30	+69·7 M.
13	W	5 44	1 27	19 56	+57·5 M.
14	Th	6 34	2 16	19 29	+45·3 M.
15	F	7 32	3 4	18 8	+33·1 M.
16	S	8 32	3 52	15 56	+20·9 M.
17	**S**	9 39	4 40	12 57	+ 8·8 M.
18	M	10 47	5 28	9 18	− 3·3 M.
19	Tu	11 59	6 16	5 6	−15·4 M.
20	W	13 13	7 4	0 32 S	−27·6 M.
21	Th	14 31	7 55	4 14 N	−39·8 M.
22	F	15 50	8 47	8 54	−52·0 M.
23	S	17 12	9 43	13 9	−64·1 M.
24	**S**	18 34	10 43	16 38	−76·3 M.
25	M	rises	11 45	18 58	−88·4 M.
26	Tu	4 42	12 48	19 56	+79·4 E.
27	W	5 45	13 50	19 27	+67·2 E.
28	Th	6 56	14 49	17 41	+55·0 E.
29	F	8 10	15 45	14 51	+42·8 E.
30	S	9 24	16 36	11 18 N	+30·7 E.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	10 35	17 25	7 17 N	+18·6 E.
2	M	11 44	18 10	3 4 N	+ 6·4 E.
3	Tu	12 53	18 54	1 10 S	− 5·7 E.
4	W	13 58	19 37	5 16	−17·9 E.
5	Th	15 3	20 21	9 6	−30·0 E.
6	F	16 6	21 5	12 32	−42·2 E.
7	S	17 8	21 50	15 27	−54·3 E.
8	**S**	18 7	22 36	17 43	−66·5 E.
9	M	19 4	23 24	19 15	−78·7 E.
10	Tu	sets	……	19 56	+89·2 M.
11	W	4 30	0 12	19 44	+77·0 M.
12	Th	5 25	1 1	18 36	+64·8 M.
13	F	6 25	1 50	16 37	+52·6 M.
14	S	7 30	2 38	13 50	+40·4 M.
15	**S**	8 38	3 26	10 22	+28·2 M.
16	M	9 48	4 13	6 22	+16·1 M.
17	Tu	10 58	5 0	1 59 S	+ 4·0 M.
18	W	12 13	5 48	2 37 N	− 8·1 M.
19	Th	13 28	6 37	7 11	−20·3 M.
20	F	14 45	7 30	11 29	−32·4 M.
21	S	16 4	8 25	15 12	−44·6 M.
22	**S**	17 21	9 24	18 1	−56·8 M.
23	M	18 32	10 25	19 38	−68·9 M.
24	Tu	19 33	11 28	19 53	−81·1 M.
25	W	rises	12 29	18 43	+86·8 E.
26	Th	5 41	13 28	16 20	+74·6 E.
27	F	6 58	14 24	13 0	+62·4 E.
28	S	8 13	15 15	9 3	+50·3 E.
29	**S**	9 26	16 3	4 46	+38·1 E.
30	M	10 35	16 49	0 25 N	+25·9 E.
31	Tu	11 44	17 34	3 50 S	+13·8 E.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. DENNING.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α °	δ °			α °	δ °	
Jan. 2-3..	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−12**	Slow; long.
3 ...	156	+41	Swift.	July-Sept..	335	+73	Swift.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	14 ...	311	+62	Rather swift.
29 ...	213	+52	Very swift.	15 ...	292	+53	Swift; bright.
Feb. 5-10	74	+43	Slow; bright.	16 ...	61	+48	Swift; streaks.
15 ...	236	+11	Swift; streaks.	21-25	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Aug.-Sept..	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	5-15	62	+37	Swift; streaks.
Mar. 1-4...	166	+ 4	Slow; bright.	6-17	106	+52	Swift; streaks.
14 ...	250	+54	Swift.	10 ...	74	+41	Swift; streaks.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow fireballs.	Oct. 2 ...	230	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
18-22	296	± 0	Swift; streaks.	4 ...	310	+77	Slowish.
18-23	189	−31	Slow; long.	8 ...	77	+31	Swift; streaks.
20-21	**270**	**+33**	Swift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	18-20	**92**	**+15**	Swift; streaks.
May 1 ...	239	+46	Small; short.	20 ...	107	+12	Swift; streaks.
5-17	254	−21	Slowish.	23 ...	99	+13	Swift; streaks.
6 ...	**338**	**− 2**	Swift; streaks.	29 ...	109	+23	Very swift.
7 ...	244	+ 7	Slow; bright.	Nov. 1 ...	43	+22	Slow; bright.
11 ...	231	+27	Slow; small.	2 ...	55	+ 9	Slow; bright.
29 ...	264	+64	Slowish.	10-12	133	+31	Very swift; streaks.
30 ...	333	+27	Swift; streaks.	14-16	**150**	**+22**	Swift; streaks.
June 2-17..	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
10 ...	261	+ 5	Very slow.	20-23	62	+23	Slow; bright.
11-19	274	+69	Rather swift.	23-24	**25**	**+44**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-28	208	+43	Very swift.
15 ...	291	+52	Swift; small.	30 ...	190	+58	Swift; streaks.
20 ...	335	+57	Swift.	Dec. 4 ...	162	+58	Swift; streaks.
26 ...	354	+39	Swift; streaks.	6 ...	80	+23	Slow; bright.
July 4 ...	303	+24	Swift.	8 ...	145	+ 7	Swift; streaks.
7-Aug.	333	+27	Swift.	8 ...	208	+71	Rather swift.
6-12	282	−13	Very slow.	10-12	**108**	**+33**	Swift; short.
15-31	23	+43	Swift; streaks.	12 ...	119	+29	Rather swift.
19 ...	314	+48	Swift; short.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November may perhaps furnish plentiful displays in 1901 and 1902.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α °	δ °		α °	δ °		α °	δ °
July 20 ...	19·7	+50·9	July 30 ...	30·8	+53·8	Aug. 9 ...	42·6	+56·5
22 ...	21·8	+51·6	Aug. 1 ...	33·2	+54·4	11 ...	45·0	+57·0
24 ...	24·0	+52·2	3 ...	35·5	+55·0	13 ...	47·5	+57·5
26 ...	26·2	+52·8	5 ...	37·9	+55·5	15 ...	50·1	+58·0
28 ...	28·5	+53·3	7 ...	40·2	+56·0	17 ...	52·6	+58·5

1901.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1..	17 53 10	24 4 S.	4·8	19 26	23 14	3 2	16 40 3	20 55 S.	12·0	17 50	21 59	2 8	10 57 32	10 9 N.	10·0	9 18	16 13	23 8
11...	19 1 39	24 15	4·6	19 56	23 43	3 29	17 33 10	22 28	11·6	18 14	22 13	2 12	11 1 6	10 9	11·0	8 42	15 37	22 32
21...	20 12 14	22 5	4·8	20 10	0 12	4 14	18 27 11	22 55	11·2	18 32	22 28	2 24	11 0 3	10 37	11·8	8 0	14 57	21 54
31...	21 22 56	17 21	5·0	20 11	0 43	5 15	19 21 8	22 12	11·0	18 41	22 42	2 43	10 53 58	11 33	12·8	7 9	14 11	21 13
Feb. 10...	22 29 42	10 20	5·6	19 59	1 10	6 21	20 14 8	20 22	10·8	18 44	22 56	3 8	10 43 10	12 51	13·4	6 14	13 21	20 30
20...	23 18 17	3 0	7·2	19 31	1 19	7 7	21 5 32	17 33	10·4	18 38	23 8	3 38	10 28 53	14 17	13·8	5 10	12 27	19 44
Mar. 2...	23 20 56	0 25	9·8	18 42	0 42	6 42	21 55 4	13 54	10·2	18 26	23 18	4 10	10 13 38	15 35	13·8	4 9	11 33	18 57
12...	22 48 36	4 21	10 8	17 44	23 24	5 4	22 42 49	9 37	10·2	18 11	23 26	4 41	10 0 12	16 30	13·2	3 10	10 40	18 10
22...	22 38 34	7 56	9·4	17 16	22 39	4 2	23 29 12	4 54 S.	10·0	17 54	23 33	5 12	9 50 43	16 56	12·4	2 19	9 52	17 25
Apr. 1...	23 1 17	7 46	8·0	17 0	22 24	3 48	0 14 47	0 2 N.	9·8	17 35	23 39	5 43	9 46 14	16 52	11·4	1 36	9 8	16 40
11...	23 43 0	4 30 S.	6·8	16 48	22 28	4 8	1 0 15	5 0	9·8	17 16	23 45	6 14	9 46 41	16 24	10·6	0 59	8 29	15 59
21...	0 36 2	1 4 N.	6·0	16 32	22 42	4 52	1 46 16	9 48	9·8	16 57	23 51	6 45	9 51 30	15 35	9·8	0 30	7 55	15 20
May 1...	1 39 16	8 19	5·4	16 20	23 7	5 54	2 33 26	14 12	9·8	16 39	23 59	7 15	9 59 57	14 28	9·0	0 5	7 24	14 45
11...	2 55 26	16 19	5·0	16 14	23 46	7 18	3 22 15	18 3	9·8	16 27	0 8	7 49	10 11 15	13 7	8·4	23 45	6 56	13 57
21...	4 23 38	22 54	5·2	16 15	0 30	8 45	4 12 55	21 6	9·8	16 18	0 19	8 20	10 24 50	11 32	7·8	23 27	6 30	13 33
31...	5 48 44	25 34	6·0	16 42	1 16	9 50	5 5 16	23 11	9·8	16 17	0 32	8 47	10 40 13	9 45	7·2	23 13	6 6	12 59
June 10...	6 55 25	24 35	7·2	17 17	1 43	10 9	5 58 44	24 9	10·0	16 24	0 46	9 8	10 57 1	7 48	6·8	23 1	5 44	12 27
20...	7 37 26	21 41	8·8	17 41	1 45	9 49	6 52 29	23 57	10·0	16 39	1 0	9 21	11 14 59	5 42	6·4	22 50	5 22	11 54
30...	7 50 7	18 40	10·8	17 34	1 18	9 2	7 45 29	22 34	10·2	17 3	1 14	9 25	11 33 57	3 28	6·2	22 41	5 2	11 23
July 10...	7 33 14	17 7	11·8	16 48	0 22	7 56	8 36 56	20 7	10·6	17 32	1 26	9 20	11 53 47	1 8 N.	5·8	22 34	4 43	10 52
20...	7 9 41	17 50	10·6	15 36	23 14	6 52	9 26 24	16 43	10·8	18 3	1 36	9 9	12 14 28	1 17 S.	5·6	22 27	4 24	10 21
30...	7 15 54	19 44	8·2	14 54	22 45	6 36	10 13 49	12 36	11·2	18 34	1 44	8 54	12 35 57	3 46	5·4	22 22	4 6	9 50
Aug. 9..	8 4 7	20 21	6·2	15 2	22 58	6 54	10 59 27	7 55	11·6	19 6	1 50	8 34	12 58 15	6 16	5·2	22 17	3 49	9 21
19...	9 20 15	17 9	5·2	15 59	23 36	7 13	11 43 49	2 54 N.	12·0	19 36	1 55	8 14	13 21 27	8 46	5·2	22 14	3 33	8 52
29...	10 37 22	10 32	4·8	17 10	0 9	7 8	12 27 33	2 15 S.	12·4	20 7	2 0	7 53	13 45 34	11 14	5·0	22 11	3 17	8 23
Sept. 8...	11 44 13	2 46 N.	4·8	18 19	0 37	6 55	13 11 19	7 22	13·0	20 37	2 4	7 31	14 10 42	13 38	4·8	22 10	3 3	7 56
18...	12 42 42	4 47 S.	5·0	19 18	0 56	6 34	13 55 47	12 15	13·8	21 8	2 9	7 10	14 36 55	15 54	4·8	22 10	2 50	7 30
28...	13 35 40	11 30	5·6	20 4	1 9	6 14	14 41 30	16 41	14·4	21 40	2 15	6 50	15 4 16	18 0	4·6	22 11	2 38	7 5
Oct. 8...	14 23 20	16 56	6·2	20 44	1 18	5 52	15 28 48	20 29	15·4	22 11	2 23	6 35	15 32 47	19 54	4·6	22 12	2 27	6 42
18 .	14 59 46	20 20	7·4	21 2	1 15	5 28	16 17 45	23 27	16·4	22 42	2 33	6 24	16 2 29	21 32	4·4	22 12	2 17	6 22
28...	15 4 25	19 51	9·2	20 25	0 40	4 55	17 7 56	25 25	17·6	23 5	2 43	6 21	16 33 18	22 51	4·4	22 13	2 9	6 5
Nov. 7...	14 24 5	13 40	9·6	18 21	23 13	4 5	17 58 27	26 16	19·2	23 23	2 55	6 27	17 5 7	23 48	4·4	22 12	2 1	5 50
17 ..	14 16 24	11 8	7·4	17 26	22 32	3 38	18 48 7	25 57	21·0	23 31	3 5	6 39	17 37 47	24 22	4·4	22 9	1 54	5 39
27...	14 57 20	14 51	5·8	17 50	22 36	3 22	19 35 30	24 33	23·0	23 29	3 13	6 57	18 11 2	24 29	4·2	22 4	1 48	5 32
Dec. 7...	15 55 12	19 33	5·0	18 37	22 55	3 13	20 19 18	22 13	25·8	23 16	3 17	7 18	18 44 36	24 9	4·2	21 55	1 42	5 29
17...	16 59 28	23 7	4·8	19 25	23 20	3 15	20 58 22	19 9	29·0	22 57	3 17	7 37	19 18 13	23 21	4·2	21 43	1 36	5 29
27...	18 7 41	24 49 S.	4·6	20 8	23 50	3 32	21 31 24	15 37 S.	33·2	22 29	3 10	7 51	19 51 36	22 7 S.	4·2	21 29	1 30	5 31

Mercury.

	d h	d h	d h
Gr. elong. E.	Feb. 19 10;	June 15 17;	Oct. 12 5
„ W.	Apr. 3 18;	Aug. 2 2;	Nov. 20 17
Sup. conj. ...	Jan. 21 14;	May 14 6;	Aug. 27 9
Inf. conj. ...	Mar. 7 3;	July 13 0;	Nov. 4 6
Stationary...	Feb. 25 10;	Mar. 19 15:	June 29 0
	July 23 15;	Oct. 24 5;	Nov. 13 1

Venus.

	d h
Gr. elong. E....	Dec. 4 22
Sup. conj.	Apr. 30 13

Superior Planets.

	Conjunction.	Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h
Mars ...	......	Feb. 21 18	May 28 8		Jan. 13 22;	Apr. 4 23
Jupiter ...	......	June 30 5	Apr. 1 18;	Sept. 27 19	Apr. 30 10;	Aug. 30 9
Saturn ...	......	July 5 9	Apr. 6 2;	Oct. 3 12	Apr. 25 17;	Sept. 14 0
Uranus ...	Dec. 9 9	June 5 20	Mar. 7 14;	Sept. 5 19	Mar. 22 0;	Aug. 22 3
Neptune .	June 20 13	Dec. 22 2	Mar. 17 6;	Sept. 24 19	Mar. 7 19;	Oct. 4 18

	Jupiter (♃).						**Saturn (♄).**					
1901.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	m h	h m s	° ′	″	h m	h m	h m
Jan. 1	17 42 55	23 5 S.	29·2	19 4	22 58	2 52	18 33 39	22 37 S.	14·0	19 51	23 48	3 45
21	18 1 59	23 10	30·0	18 6	21 59	1 52	18 43 42	22 29	14·0	18 41	22 40	2 39
Feb. 10	18 19 34	23 7	31·0	17 3	20 57	0 51	18 52 58	22 19	14·2	17 30	21 30	1 30
Mar. 2	18 34 45	22 58	32·4	15 59	19 54	23 49	19 0 49	22 9	14·6	16 18	20 19	0 20
22	18 46 36	22 48	34·2	14 51	18 47	22 43	19 6 40	22 1	15·0	15 4	19 6	23 8
Apr. 11	18 54 10	22 41	36·4	13 39	17 35	21 31	19 10 5	21 56	15·4	13 49	17 51	21 53
May 1	18 56 41	22 39	38·8	12 22	16 19	20 16	19 10 44	21 55	16·0	12 31	16 33	20 35
21	18 53 51	22 45	41·2	11 1	14 57	18 53	19 8 41	21 59	16·4	11 10	15 12	19 14
June 10	18 46 15	22 57	42·8	9 37	13 31	17 25	19 4 18	22 7	16·8	9 48	13 49	17 50
30	18 35 41	23 10	43·4	8 9	12 2	15 55	18 58 24	22 18	17·0	8 26	12 25	16 24
July 20	18 25 2	23 20	43·0	6 41	10 33	14 25	18 52 9	22 28	17·0	7 2	11 0	14 58
Aug. 9	18 17 10	23 27	41·4	5 15	9 6	12 57	18 46 48	22 36	16·6	5 39	9 36	13 33
29	18 14 5	23 30	39·2	3 54	7 45	11 36	18 43 22	22 42	16·2	4 18	8 14	12 10
Sept. 18	18 16 26	23 32	37·0	2 39	6 29	10 19	18 42 32	22 45	15·8	2 59	6 55	10 51
Oct. 8	18 23 58	23 30	34·8	1 26	5 17	9 8	18 44 31	22 45	15·2	1 42	5 38	9 34
28	18 35 53	23 23	33·0	0 19	4 11	8 3	18 49 9	22 42	14·8	0 28	4 24	8 20
Nov. 17	18 51 13	23 8	31·6	23 14	3 7	7 0	18 56 4	22 35	14·4	23 14	3 12	7 10
Dec. 7	19 8 58	22 45	30·6	22 10	2 6	6 2	19 4 40	22 25	14·2	22 3	2 2	6 1
27	19 28 13	22 10 S.	30·0	21 7	1 7	5 7	19 14 22	22 10 S.	14·0	20 53	0 53	4 53

	Uranus (♅).						**Neptune (♆).**					
Jan. 1	16 52 2	22 31 S.	3·4	18 9	22 7	2 5	5 49 14	22 11 N.	2·7	2 58	11 6	19 14
31	16 58 26	22 42	3·5	16 18	20 15	0 12	5 46 9	22 11	2·7	0 57	9 5	17 13
Mar. 2	17 2 19	22 47	3·6	14 25	18 21	22 17	5 44 41	22 12	2·6	22 57	7 5	15 13
Apr. 1	17 2 53	22 49	3·6	12 29	16 24	20 19	5 45 22	22 13	2·6	21 0	5 8	13 16
May 1	17 0 16	22 45	3·7	10 27	14 23	18 19	5 48 4	22 15	2·6	19 5	3 13	11 21
31	16 55 29	22 38	3·7	8 23	12 20	16 17	5 52 13	22 17	2·5	17 11	1 19	9 27
June 30	16 50 21	22 30	3·7	6 19	10 17	14 15	5 57 0	22 18	2·5	15 14	23 22	7 30
July 30	16 46 42	22 25	3·7	4 18	8 16	12 14	6 1 30	22 18	2·5	13 20	21 28	5 36
Aug. 29	16 45 50	22 23	3·6	2 18	6 17	10 16	6 4 51	22 17	2·6	11 26	19 34	3 42
Sept. 28	16 48 13	22 28	3·5	0 43	4 21	7 59	6 6 24	22 16	2·6	9 29	17 37	1 45
Oct. 28	16 53 30	22 37	3·4	22 32	2 29	6 26	6 5 49	22 15	2·6	7 31	15 39	23 47
Nov. 27	17 0 43	22 47	3·3	20 42	0 38	4 34	6 3 19	22 15	2·7	5 30	13 38	21 46
Dec. 27	17 8 34	22 58 S.	3·4	18 49	22 44	2 39	5 59 46	22 15 N.	2·7	3 29	11 37	19 45

	Ceres.				**Pallas.**		
1901.	R.A. Noon.	Dec. Noon.	Transits.	1900.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Oct. 5 ...	2 44	3 10 N.	13 47	Aug. 18 ...	23 7	5 27 N.	13 20
17 ...	2 35	2 31	12 52	30 ...	22 59	3 14	12 25
29 ...	2 25	2 0	11 54	Sept. 11 ...	22 50	0 41 N.	11 28
Nov. 10 ...	2 14	1 42	10 56	23 ...	22 41	1 59 S.	10 33
22 ...	2 5	1 44 N.	10 0	Oct. 5 ...	22 34	4 32 S.	9 39

	Vesta.		
Jan. 14 ...	9 25	20 15 N.	13 49
26 ...	9 14	21 46	12 51
Feb. 7 ...	9 2	23 14	11 52
19 ...	8 50	24 26 N.	10 53

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1901.

In the year 1901 there will be two Eclipses of the Sun and two of the Moon (one being only penumbral).

I.—*A Penumbral Eclipse of the Moon*, 1901, *May* 3.

Partly visible at Greenwich. First contact with the penumbra occurs at 62° from the North Point towards the East. Last contact at 38° towards the West.

	h	m
First contact with Penumbra	4	6·3
Last " " "	8	55·1

The Moon will rise at Greenwich at 7^{h} 28^{m}.

II.—*A Total Eclipse of the Sun*, 1901, *May* 17.

Invisible at Greenwich. The centre of the shadow begins its course in the Indian Ocean west of Madagascar, skirts the southern coast of that island and crosses Reunion and Mauritius. It next touches land in the Malay Archipelago, passes through Sumatra and Borneo, and ends its course East of New Guinea, where the Sun will be setting.

	Eclipse begins.			Totality begins.			Totality ends.			Sun's Altitude.
	d	h	m	h	m	s	h	m	s	°
Mauritius (Observatory)......	17	15	1	16	1	36	16	5	11	19
Sumatra (Padang)	17	16	4	17	37	27	17	43	41	69
Borneo (Pontianak)	17	16	31	18	5	56	18	11	36	61
Amboyna (Port Victoria) ...	17	17	27	18	48	28	18	52	43	34
New Guinea (Port Moresby).	17	18	1	19	6	39	19	9	58	11

G.M.T. in all cases.

Central Eclipse begins May 17^{d} 15^{h} $57^{m}\cdot7$ G.M.T. in Long. 40° 6′ E., Lat. 27° 30′ S.

Central Eclipse ends May 17^{d} 19^{h} $10^{m}\cdot1$ G.M.T. in Long. 156° 58′ E., Lat. 12° 50′ S.

A Partial Eclipse will be visible at the following places. The times are local Mean in all cases:—

	Eclipse begins.			Greatest Eclipse.		Eclipse ends.		Magnitude.
	d	h	m	h	m	h	m	
Madras	17	21	15·9	22	18·7	23	28·0	0·367
Hong Kong	18	1	0·6	1	59·3	2	53·4	0·272
Perth (W. Aust.)	18	0	57·0	1	48·7	2	38·0	0·197
Adelaide..................	18	3	8·7	3	55·6	4	39·3	0·250

III.—*A Partial Eclipse of the Moon*, 1901, *October* 27.

Partly visible as a penumbral eclipse at Greenwich. Magnitude of the Eclipse ·227 (Moon's diam. =1). The first contact with the shadow occurs at 137° from the North point towards the East. Last contact at 166° towards the West.

	h	m		h	m
First contact with Penumbra	1	5·2	With shadow	2	25·0
Last " " "	5	25·6	" "	4	5·8

The Moon will rise at Greenwich at 4^{h} 35^{m}.

IV.—*An Annular Eclipse of the Sun*, 1901, *November* 10.

Invisible at Greenwich. The central line passes from South of Sicily, through the Mediterranean, Suez, Arabia, Ceylon, Malay, Assam, and the Philippines.

Central Eclipse begins generally November 10^{d} 17^{h} $43^{m}\cdot8$ G.M.T. in Longitude 13° 29′ E. of Greenwich, Lat. 36° 57′ N. Ends generally November 10^{d} 21^{h} $13^{m}\cdot0$ in Long. 122° 12′ East of Greenwich, Lat. 17° 23′ N.

At Madras a Partial Eclipse will be visible. Magnitude 0·846 (Sun's diam. =1). Eclipse begins 10^{d} 23^{h} $17^{m}\cdot6$. Eclipse ends 11^{d} 3^{h} $22^{m}\cdot2$ (Madras time). Angle from North point. First contact 79° W. Last contact 105° E.

OCCULTATIONS, 1901. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
Jan.			h m	°	h m	°
1	B.A.C. 1242 ...	6·6	9 8	76	10 22	268
5	1 Cancri	5·9	9 45	129	10 50	258
5	B.A.C. 2737 ...	6·3	17 55	121	18 51	278
6	A^1 Cancri	5·6	6 36	108	7 31	277
6	60 Cancri	5·7	13 48	123	15 4	287
11	B.A.C. 4312 ...	6·5	15 49	130	17 9	286
14	ι^2 Libræ	6·5	14 47†	116	15 53	280
28	13 Tauri.........	5·4	8 3	9	8 28	331
28	14 Tauri.........	6·4	8 32	43	9 28	302
29	D.M.+20° 785 .	5·8	6 0	76	6 37	268
30	B.A.C. 1733 ...	6·3	4 15	150	4 42	202
30	χ^1 Orionis	4·7	14 41	110	15 36	268
Feb.						
1	D.M.+17° 1596	5·6	6 32	146	7 18	231
2	A^1 Cancri	5·6	17 20	43	17 42	357
2	A^2 Cancri	5·8	18 57‡	107	19 47†	288
3	ω Leonis.........	5·6	14 34	88	15 37	324
5	p^2 Leonis	6·2	15 14	123	16 30	294
9	B.A.C. 4700 ...	5·3	17 38	133	18 53	264
10	10 Libræ.........	6·4	12 36†	118	13 42	281
13	52 Ophiuchi ...	6·5	16 23	99	17 36	268
13	B.A.C. 5954 ...	6·3	18 25	61	19 36	298
14	D.M.−20° 5189	6·5	18 57	67	20 12	280
21	51 Piscium......	5·7	6 54	76	7 51	245
22	π Piscium	5·6	10 20	97	11 7†	237
24	B.A.C. 1064 ...	6·5	7 17	51	8 17	293
26	B.A.C. 1733 ...	6·3	11 21	87	12 21	288
Mar.						
2	κ Cancri	5·0	10 46	122	12 3	289
13	B.A.C. 6125 ...	6·4	15 12	118	16 18	242
24	ω^1 Tauri	5·8	7 37	89	8 40	268
25	*l* Tauri	5·5	6 32	84	7 42	282
25	B.A.C. 1639 ...	6·5	11 44	94	12 35	276
26	68 Orionis	5·6	9 15	63	10 6	319
28	1 Cancri	5·9	6 16	34	6 40	359
29	A^2 Cancri	5·8	5 30	142	6 33	256
Apr.						
4	B.A.C. 4531 ...	5·7	11 59	97	13 18	311
7	ω^2 Scorpii	4·6	15 59	81	17 19	288
7	ω^1 Scorpii	4·1	16 5	16	16 20	356
8	D.M.−210 4449	6·5	13 37	86	14 58	286
10	D.M.−20° 5223	6·5	13 14	36	13 58	319
22	D.M.+19° 1110	6·1	9 26	65	10 11	312
22	χ^2 Orionis	5·9	10 25	71	11 11	305
23	D.M.+18° 1349	6·5	8 6	178	8 24	210
27	19 Sextantis ...	6·0	7 40	168	8 31	247
28	p^2 Leonis	6·2	10 44	96	11 53	316
May						
1	*i* Virginis	5·5	15 18	86	16 19†	304
7	21 Sagittarii ...	4·9	12 39	62	13 51	291
8	*d* Sagittarii ...	4·9	12 26	122	13 23	222
13	λ Piscium	4·7	13 57†	90	14 48	227
31	B.A.C. 5109 ...	5·4	11 49	44	12 36	336
June			h m	°	h m	
3	*p* Sagittarii......	4·1	14 4	111	15 13	
3	15 Sagittarii ...	5·6	15 6	48	16 12	
4	D.M.−19° 5273	5·9	11 52	31	12 41	
4	B.A.C. 6536 ...	5·5	14 49	102	15 59	
7	c^1 Capricorni...	5·2	14 59	109	15 54	
25	B.A.C. 4531 ...	5·7	7 57	101	9 18	
28	ω^1 Scorpii	4·1	11 15	53	12 15	
28	ω^2 Scorpii	4·6	11 30	100	12 48	
29	D.M.−21° 4449	6·5	8 45	96	10 7	
July						
1	D.M.−20° 5223	6·5	7 26†	44	8 14	
2	B.A.C. 6710 ...	6·0	9 29	157	9 47	
3	B.A.C. 7063 ...	6·2	9 59	151	10 17	
18	19 Sextantis ...	6·0	8 47	156	9 25†	
28	21 Sagittarii ...	4·9	9 51	60	11 6	
29	*d* Sagittarii ...	4·9	8 33	96	9 48	
Aug.						
2	D.M.−3° 5539 .	6·3	12 58	45	14 8	
4	δ Piscium	4·6	14 39	84	15 47	
6	29 Arietis	6·5	10 51	57	11 42	
7	D.M +17° 564 .	6·5	11 11	26	11 43	
24	B.A.C. 6098 ...	6·5	8 47	44	9 51	
25	D.M.−19° 5273	5·9	12 39	109	13 31†	
28	c^1 Capricorni...	5·2	12 54	85	13 59	
29	κ Aquarii	5·5	12 29	28	13 29	
Sept.						
4	B.A.C. 1361 ...	6·5	14 9	121	15 0	
4	ε Tauri	3·7	16 2	123	16 58	
5	B.A.C. 1647 ...	6·5	12 7	90	13 2	
5	B.A.C. 1651 ...	6·5	12 33	37	13 12	
6	71 Orionis	5·1	10 48†	31	11 13†	
9	κ Cancri	5·0	14 32†	90	15 25	
26	D.M.−3° 5539 .	6·3	7 5	347	7 16	
27	25 Piscium......	6·3	6 20	116	6 59	
28	62 Piscium......	6·0	6 1	29	6 41	
Oct.						
1	B.A.C. 1240 ...	5·7	11 14	129	11 53	
3	71 Orionis	5·1	17 10	58	18 9	
4	D.M.+17° 1479	6·2	12 3	35	12 33	
6	60 Cancri	5·7	14 46	39	15 12	
17	ξ Ophiuchi......	4·5	5 2	149	5 38	
18	16 Sagittarii ...	6·2	6 39	119	7 36	
19	D.M.−18° 5206	6·5	6 50	86	8 5	
22	c^1 Capricorni...	5·2	9 5	153	9 7	
23	κ Aquarii	5·5	8 53	61	10 6	
24	λ Piscium	4·7	14 35	73	15 30	
27	29 Arietis	6·5	12 15	31	13 8	
27	o Arietis	5·8	17 32	108	18 21	
31	D.M.+18° 1349	6·5	13 11	62	14 10	

† Star below horizon. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

Date.	Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.	
			M. T.	P.	M. T.	P.				M. T.	P.	M. T.	P.
Nov.			h m	°	h m	°	**Dec.**			h m	°	h m	°
2	A^2 Cancri	5·8	17 32	42	17 59	1	14	B.A.C. 6992 ...	6·7	6 36	38	7 31‡	282
3	ω Leonis	5·6	12 4	41	12 30	348	14	β Capricorni ...	3·4	6 43	42	7 39†	278
14	B.A.C. 6081 ...	6·5	5 54	58	6 57†	282	17	D.M.−3° 5539.	6·3	10 53†	87	11 46†	233
22	62 Piscium......	6·0	3 42	42	4 34	274	18	λ Piscium	4·7	7 24	11	8 9	297
24	*o* Arietis	5·8	3 15	29	3 49	300	21	29 Arietis	6·5	10 15	16	10 52	313
25	B.A.C. 1240 ...	5·7	8 11	117	9 0	218	21	*o* Arietis	5·8	15 11	81	16 2‡	263
25	D.M.+18° 624.	5·9	17 14	105	18 7	255	23	B.A.C. 1361 ...	6·5	4 30	6	4 45	333
25	B.A.C. 1361 ...	6·5	19 5	34	19 32	326	23	ε Tauri	3·7	5 53	9	6 12	330
26	B.A.C. 1647 ...	6·5	15 4	69	16 4	299	25	D.M.+18° 1349	6·5	10 19	85	11 29	289
27	D.M.+18° 1129	6·5	9 52	140	10 33	220	27	A^2 Cancri	5·8	10 42	105	11 50	287
27	71 Orionis	5·1	10 41	12	10 54	350							
28	68 Geminorum.	5·0	19 18	142	20 6	251							
30	κ Cancri	5·0	9 54	46	10 24	341							

† Star below horizon. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively :—

North Near Approaches.

Date.		Star.	Mag.	G.M.T.	Angle.	Date.		Star.	Mag.	G.M.T.	Angle.
				h m	°					h m	°
Jan.	1	ω^2 Tauri	4·6	17 1	0	Sept.	23	B.A.C. 6992 ...	6·7	9 18	340
	27	π Arietis	5·6	11 1	351		23	β^2 Capricorni ...	3·4	9 26	340
Feb.	28	D.M.+17°1596	5·6	14 48	16		28	δ Piscium	4·6	6 40	339
Mar.	28	B.A.C. 2737 ...	6·3	14 22	17	Oct.	2	B.A.C. 1563 ...	6·5	13 8	353
	29	60 Cancri	5·7	11 36	23		29	B.A.C. 1361 ...	6·5	6 15	351
May	2	B.A.C. 4679 ...	6·5	9 12	23		29	ε Tauri	3·7	7 34	351
	4	λ Libræ	5·0	16 16	3	Nov.	5	p^5 Leonis	5·5	17 29	26
	8	ρ^2 Sagittarii ...	6·1	15 44	347		17	B.A.C. 7087 ...	6·2	7 51	338
	15	π Piscium	5·6	15 38	341		22	δ Piscium	4·6	4 30	337
July	29	ρ^2 Sagittarii ...	6·1	12 3	344	Dec.	15	ν Aquarii	4·6	6 32	336
Aug.	3	λ Piscium	4·7	8 42	340		25	D.M.+17°1479	6·2	18 2	13
	3	22 Piscium......	5·9	14 8	333		27	60 Cancri	5·7	16 13	23
	8	B.A.C. 1417 ...	6·5	13 25	351		29	36 Sextantis ...	6·5	19 13	25

South Near Approaches.

				h m	°					h m	°
Jan.	3	χ^1 Orionis	4·7	5 55	178	Aug.	4	62 Piscium ...	6·0	14 45	154
	3	χ^4 Orionis	4·8	10 47	185		28	c^2 Capricorni...	6·2	13 52	155
	3	68 Orionis	5·6	15 10	191	Sept.	4	D.M.+18°624.	5·9	12 26	169
	6	A^2 Cancri	5·8	9 3	195		29	B.A.C. 609 ...	6·2	11 56	158
	24	22 Piscium ...	5·9	5 10	153	Oct.	1	D.M.+18°594.	6·5	16 25	174
Feb.	5	55 Leonis	6·0	10 26	205		3	D.M.+18°1129	6·5	17 4	185
	25	B.A.C. 1417 ...	6·5	12 6	182	Nov.	21	25 Piscium ...	6·3	3 36	157
Mar.	1	B.A.C. 2737 ...	6·3	7 23	195		25	D M.+18°594.	6·5	13 26	175
	10	D.M.−19°4106	6·5	13 58	197		28	λ Geminorum ..	3·6	11 39	187
	26	71 Orionis	5·1	11 11	191	Dec.	1	19 Sextantis ...	6·0	18 56	206
Apr.	4	*i* Virginis	5·5	7 39	202		2	p^2 Leonis	6·2	20 38	206
	26	3 Leonis	6·0	10 23	205		23	*i* Tauri	5·1	16 28	182
May	3	10 Libræ	6·4	9 12	200						

ILLUMINATED DISK OF MERCURY.

At Greenwich Noon.

Date.	*k*	*θ*	Date.	*k*	*θ*	Date.	*k*	*θ*
Jan. 1...	0·949	178°·2	May 1...	0·869	151°·6	Sept. 8...	0·952	18°·0
11...	·985	160·8	11...	·992	165·2	18...	·884	23·5
21...	·998	91·0	21...	·941	342·0	28...	·806	24·9
31...	·975	357·9	31...	·718	352·4	Oct. 8...	·686	24·2
Feb. 10...	·837	341·8	June 10...	·495	1·5	18...	·486	23·5
20...	·467	332·8	20...	·307	9·2	28...	·162	25·0
Mar. 2...	·072	315·7	30...	·134	17·2	Nov. 7...	·032	224·5
12...	·048	177·6	July 10...	·019	54·1	17...	·453	206·1
22...	·260	158·8	20...	·062	162·5	27...	·778	202·4
April 1...	·452	154·7	30...	·292	179·8	Dec. 7...	·915	195·4
11...	·602	151·9	Aug. 9...	·645	191·7	17...	·973	183·3
21...	0·731	150·3	19...	·936	207·2	27...	0·996	155·1
			29...	0·996	333·7			

k is the ratio of the illuminated portion of the apparent disk to the entire apparent disk considered as the superficies of a circle.

θ is the angle which the line joining the cusps, or extremities of the illuminated portion makes with the meridian.

APPARENT DISKS OF VENUS AND MARS, 1901.

	Venus.	*Mars.*		*Venus.*	*Mars.*		*Venus.*	*Mars.*
Jan. 15...	0·902	0·949	May 15...	0·998	0·895	Sept. 15...	0·795	0·933
Feb. 14...	·949	·997	June 15...	·977	·890	Oct. 15...	·705	·951
Mar. 15...	·980	·979	July 15...	·936	·900	Nov. 15...	·591	·967
April 15...	·998	·924	Aug. 15...	·873	·915	Dec. 15...	·445	·980

The numbers in this table are the versed sines of the illuminated disk, the apparent diameter of the planets being taken as unity.

SATELLITES OF MARS

DURING THE OPPOSITION OF 1901.

East Elongations.

PHOBOS.

February.		Feb. (*con.*).		Feb. (*con.*).		March.		Mar. (*con.*).	
d	h	d	h	d	h	d	h	d	h
2	20·4	11	18·6	20	16·8	1	15·1	12	18·9
5	1·9	14	0·2	22	22·4	3	20·6	15	0·4
7	7·5	16	5·7	25	3·9	6	2.2	17	6·0
9	13·0	18	11·3	27	9·5	8	7·7	19	11·6
						10	13·3	21	17·1

DEIMOS.

February.		Feb. (*con.*).		March.		Mar. (*con.*).		Mar. (*con.*).	
d	h	d	h	d	h	d	h	d	h
4	1·9	19	5·0	2	13·3	13	21·7	25	6·2
7	20·7	22	23·8	6	8·1	17	16·5	29	1·0
11	15·5	26	18·6	10	2·9	21	11·3		
15	10·3								

For Phobos every seventh eastern elongation is given; for Deimos every third.

JUPITER'S SATELLITES, 1901.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re. *t*, Tr. Ing.; T, Tr. Egr.

d	h	m	
January.			
11	6	51	ii. *t*.
	9	33	ii. T.
	13	12	i. *t*.
	15	28	i. T.
12	9	54	i. *e*.
	12	38	i. O.
	14	12	iii. *t*.
	16	54	iii. T.
13	1	1	ii. *e*.
	4	40	ii. O.
	7	43	i. *t*.
	9	58	i. T.
14	4	23	i. *e*.
	7	8	i. O.
	20	16	ii. *t*.
	22	57	ii. T.
15	2	13	i. *t*.
	4	28	i. T.
	22	51	i. *e*.
16	1	39	i. O.
	2	22	iii. *e*.
	7	14	iii. O.
	14	18	ii. *e*.
	18	4	ii. O.
	20	43	i. *t*.
	22	58	i. T.
17	17	20	i. *e*.
	20	9	i. O.
18	9	41	ii. *t*.
	12	23	ii. T.
	15	13	i. *t*.
	17	29	i. T.
19	11	48	i. *e*.
	14	39	i. O.
	18	37	iii. *t*.
	21	21	iii. T.
20	3	35	ii. *e*.
	7	28	ii. O.
	9	43	i. *t*.
	11	59	i. T.
21	6	17	i. *e*.
	9	9	i. O.
	23	5	ii. *t*.
22	1	47	ii. T.
	4	13	i. *t*.
	6	29	i. T.
23	0	45	i. *e*.
	3	39	i. O.
	6	20	iii. *e*.
	8	48	iii. E.
	8	56	iii. *o*.
	11	41	iii. O.
	16	52	ii. *e*.
	20	52	ii. O.
Jan. (con.).			
23	22	43	i. *t*.
24	0	59	i. T.
	19	14	i. *e*.
	22	9	i. O.
25	12	30	ii. *t*.
	15	12	ii. T.
	17	13	i. *t*.
	19	29	i. T.
26	13	42	i. *e*.
	16	40	i. O.
	23	1	iii. *t*.
27	1	47	iii. T.
	6	8	ii. *e*.
	10	15	ii. O.
	11	43	i. *t*.
	13	59	i. T.
28	8	11	i. *e*.
	11	10	i. O.
29	1	54	ii. *t*.
	4	36	ii. T.
	6	13	i. *t*.
	8	29	i. T.
30	2	39	i. *e*.
	5	40	i. O.
	10	19	iii. *e*.
	12	48	iii. E.
	13	20	iii. *o*.
	16	7	iii. O.
	19	25	ii. *e*.
	23	39	ii. O.
31	0	43	i. *t*.
	2	59	i. T.
	21	8	i. *e*.
February.			
1	0	10	i. O.
	15	18	ii. *t*.
	18	1	ii. T.
	19	12	i. *t*.
	21	28	i. T.
2	15	36	i. *e*.
	18	40	i. O.
3	3	23	iii. *t*.
	6	11	iii. T.
	8	42	ii. *e*.
	13	2	ii. O.
	13	42	i. *t*.
	15	58	i. T.
4	10	5	i. *e*.
	13	10	i. O.
5	4	41	ii. *t*.
	7	25	ii. T.
	8	12	i. *t*.
Feb. (con.).			
5	10	28	i. T.
6	4	33	i. *e*.
	7	40	i. O.
	14	16	iii. *e*.
	16	47	iii. E.
	17	42	iii. *o*.
	20	31	iii. O.
	21	59	ii. *e*.
7	2	24	ii. O.
	2	42	i. *t*.
	4	58	i. T.
	23	2	i. *e*.
8	2	10	i. O.
	18	5	ii. *t*.
	20	49	ii. T.
	21	12	i. *t*.
	23	28	i. T.
9	17	30	i. *e*.
	20	40	i. O.
10	7	44	iii. *t*.
	10	35	iii. T.
	11	16	ii. *e*.
	15	41	i. *t*.
	15	47	ii. O.
	17	58	i. T.
11	11	59	i. *e*.
	15	9	i. O.
12	7	28	ii. *t*.
	10	11	i. *t*.
	10	12	ii. T.
	12	27	i. T.
13	6	27	i. *e*.
	9	39	i. O.
	18	14	iii. *e*.
	20	46	iii. E.
	22	1	iii. *o*.
14	0	32	ii. *e*.
	0	52	iii. O.
	4	41	i. *t*.
	5	9	ii. O.
	6	57	i. T.
15	0	55	i. *e*.
	4	9	i. O.
	20	52	ii. *t*.
	23	10	i. *t*.
	23	36	ii. T.
16	1	26	i. T.
	19	24	i. *e*.
	22	38	i. O.
17	12	3	iii. *t*.
	13	49	ii. *e*.
	14	55	iii. T.
	17	40	i. *t*.
	18	30	ii. O.
Feb. (con.).			
17	19	56	i. T.
18	13	52	i. *e*.
	17	8	i. O.
19	10	14	ii. *t*.
	12	9	i. *t*.
	12	58	ii. T.
	14	25	i. T.
20	8	21	i. *e*.
	11	38	i. O.
	22	12	iii. *e*.
21	0	45	iii. E.
	2	19	iii. *o*.
	3	6	ii. *e*.
	5	11	iii. O.
	6	38	i. *t*.
	7	52	ii. O.
	8	55	i. T.
22	2	49	i. *e*.
	6	7	i. O.
	23	37	ii. *t*.
23	1	8	i. *t*.
	2	22	ii. T.
	3	24	i. T.
	21	18	i. *e*.
24	0	37	i. O.
	16	20	iii. *t*.
	16	22	ii. *e*.
	19	13	iii. T.
	19	37	i. *t*.
	21	13	ii. O.
	21	54	i. T.
25	15	46	i. *e*.
	19	7	i. O.
26	12	59	ii. *t*.
	14	7	i. *t*.
	15	44	ii. T.
	16	23	i. T.
27	10	15	i. *e*.
	13	36	i. O.
28	2	10	iii. *e*.
	4	44	iii. E.
	5	39	ii. *e*.
	6	35	iii. O.
	8	36	i. *t*.
	9	28	iii. O
	10	34	ii. O.
	10	52	i. T.
March.			
1	4	43	i. *e*.
	8	6	i. O.
2	2	21	ii. *t*.
	3	5	i. *t*.
Mar. (con.).			
2	5	7	ii. T.
	5	22	i. T.
	23	11	i. *e*.
3	2	35	i. O.
	18	56	ii. *e*.
	20	33	iii. *t*.
	21	36	i. *t*.
	23	27	iii. T.
	23	51	i. T.
	23	54	ii. O.
4	17	40	i. *e*.
	21	4	i. O.
5	15	42	ii. *t*.
	16	4	i. *t*.
	18	20	i. T.
	18	28	ii. T.
6	12	8	i. *e*.
	15	34	i. O.
7	6	8	iii. *e*.
	8	12	ii. *e*.
	8	44	iii. E.
	10	33	i. *t*.
	10	48	iii. *o*.
	12	49	i. T.
	13	14	ii. O.
	13	43	iii. O.
8	6	37	i. *e*.
	10	3	i. O.
9	5	2	i. *t*.
	5	4	ii. *t*.
	7	18	i. T.
	7	50	ii. T.
10	1	5	i. *e*.
	4	32	i. O.
	21	29	ii. *e*.
	23	31	i. *t*.
11	0	43	iii. *t*.
	1	47	i. T.
	2	34	ii. O.
	3	39	iii. T.
	19	34	i. *e*.
	23	1	i. O.
12	0	40	iv. *o*.
	1	23	iv. O.
	18	0	i. *t*.
	18	24	ii. *t*.
	20	17	i. T.
	21	10	ii. T.
13	14	2	i. *e*.
	17	31	i. O.
14	10	6	iii. *e*.
	10	45	ii. *e*.
	12	29	i. *t*.
	12	43	iii. E.
Mar. (con.).			
14	14	45	i. T.
	14	58	iii. *o*.
	15	54	ii. O.
	17	54	iii. O.
15	8	30	i. *e*.
	12	0	i. O.
16	6	57	i. *t*.
	7	45	ii. *t*.
	9	14	i. T.
	10	32	ii. T.
17	2	59	i. *e*.
	6	29	i. O.
18	0	2	ii. *e*.
	1	26	i. *t*.
	3	43	i. T.
	4	51	iii. *t*.
	5	13	ii. O.
	7	48	iii. T.
	21	27	i. *e*.
19	0	58	i. O.
	19	55	i. *t*.
	21	5	ii. *t*.
	22	12	i. T.
	23	51	ii. T.
20	9	16	iv. *t*.
	10	22	iv. T.
	15	56	i. *e*.
	19	26	i. O.
21	13	18	ii. *e*.
	14	5	iii. *e*.
	14	24	i. *t*.
	16	41	i. T.
	16	42	iii. E.
	18	31	ii. O.
	19	4	iii. *o*.
	22	2	iii. O.
22	10	24	i. *e*.
	13	55	i. O.
23	8	52	i. *t*.
	10	25	ii. *t*.
	11	9	i. T.
	13	12	ii. T.
24	4	53	i. *e*.
	8	24	i. O.
25	2	35	ii. *e*.
	3	21	i. *t*.
	5	38	i. T.
	7	49	ii. O.
	8	55	iii. *t*.
	11	54	iii. T.
	23	21	i. *e*.
26	2	53	i. O.
	21	48	i. *t*.
	23	44	ii, *t*.

Mar. (con.).

d	h	m	
27	0	6	i. T.
	2	31	ii. T.
	17	49	i. *e.*
	21	22	i. O.
28	15	51	ii. *e.*
	16	18	i. *t.*
	18	2	iii. *e.*
	18	35	i. T.
	19	0	iv. *o.*
	20	22	iv. O.
	20	41	iii. E.
	21	7	ii. O.
	23	7	iii. *o.*
29	2	6	iii. O.
	12	18	i. *e.*
	15	50	i. O.
30	10	46	i. *t.*
	13	3	i. T.
	13	4	ii. *t.*
	15	50	ii. T.
31	6	46	i. *e.*
	10	19	i. O.

April.

1	5	8	ii. *e.*
	5	15	i. *t.*
	7	32	i. T.
	10	24	ii.O.
	12	56	iii. *t.*
	15	56	iii. T.
2	1	15	i. *e.*
	4	47	i. O.
	23	43	i. *t.*
3	2	0	i. T.
	2	21	ii. *t.*
	5	8	ii. T.
	19	43	i. *e.*
	23	16	i. O.
4	18	11	i. *t.*
	18	24	ii. *e.*
	20	28	i. T.
	22	0	iii. *e.*
	23	41	ii. O.
5	0	40	iii. E.
	3	5	iii. *o.*
	6	6	iii. O.
	14	11	i. *e.*
	17	44	i. O.
6	3	20	iv. *t.*
	4	52	iv. T.
	12	39	i. *t.*
	14	56	i. T.
	15	40	ii. *t.*
	18	27	ii. T.
7	8	40	i. *e.*
	12	12	i. O.
8	7	7	i. *t.*
	7	41	ii. *e.*
	9	24	i. T.
	12	57	ii. O.
	16	53	iii. *t.*
	19	54	iii. T.
9	3	8	i. *e.*
	6	41	i. O.

Apr. (con.).

d	h	m	
10	1	36	i. *t.*
	3	53	i. T.
	4	56	ii. *t.*
	7	44	ii. T.
	21	37	i. *e.*
11	1	9	i. O.
	20	4	i. *t.*
	20	58	ii. *e.*
	22	21	i. T.
12	1	58	iii. *e.*
	2	13	ii. O.
	4	39	iii. E.
	7	0	iii. *o.*
	10	1	iii. O.
	16	5	i. *e.*
	19	37	i. O.
13	14	31	i. *t.*
	16	48	i. T.
	18	14	ii. .
	21	1	ii. T.
14	10	34	i. *e.*
	12	40	iv. *o.*
	14	5	i. O.
	14	22	iv. O.
15	8	59	i. *t.*
	10	14	ii. *e.*
	11	16	i. T.
	15	28	ii. O.
	20	45	iii. *t.*
	23	47	iii. T.
16	5	2	i. *e.*
	8	33	i. O.
17	3	27	i. *t.*
	5	44	i. T.
	7	29	ii. *t.*
	10	17	ii. T.
	23	30	i. *e.*
18	21	55	i. *t.*
	23	31	ii. *e.*
19	0	12	i. T.
	4	43	ii. O.
	5	56	iii. *e.*
	8	38	iii. E.
	10	51	iii. *o.*
	13	53	iii. O.
	17	59	i. *e.*
	21	29	i. O.
20	16	22	i. *t.*
	18	40	i. T.
	20	46	ii. *t.*
	23	33	ii. T.
21	12	27	i. *e.*
	15	57	i. O.
22	10	50	i. *t.*
	12	47	ii. *e.*
	13	7	i. T.
	17	57	ii. O.
	20	38	iv. *t.*
	22	25	iv. T.
23	0	33	iii. *t.*
	3	36	iii. T.
	6	56	i. *e.*
	10	24	i. O.
24	5	18	i. *t.*

Apr. (con.).

d	h	m	
24	7	35	i. T.
	10	0	ii. *t.*
	12	48	ii. T.
25	1	24	i. *e.*
	4	52	i. O.
	23	45	i. *t.*
26	2	2	i. T.
	2	4	ii. *e.*
	7	11	ii. O.
	9	54	iii. *e.*
	12	38	iii. E.
	14	37	iii. *o.*
	17	40	iii. O.
	19	53	i. *e.*
	23	20	i. O.
27	18	13	i. *t.*
	20	30	i. T.
	23	15	ii. *t.*
28	2	3	ii. T.
	14	21	i. *e.*
	17	47	i. O.
29	12	40	i. *t.*
	14	57	i. T.
	15	21	ii. *e.*
	20	24	ii. O.
30	4	16	iii. *t.*
	7	19	iii. T.
	8	49	i. *e.*
	12	14	i. O.
	19	15	iv. *e.*
	19	58	iv. E.

May.

1	5	31	iv. *o.*
	7	7	i. *t.*
	7	23	iv. O.
	9	24	i. T.
	12	29	ii. *t.*
	15	17	ii. T.
2	3	18	i. *e.*
	6	42	i. O.
3	1	34	i. *t.*
	3	52	i. T.
	4	37	ii. *e.*
	9	37	ii. O.
	13	52	iii. *e.*
	16	38	iii. E.
	18	19	iii. *o.*
	21	22	iii O.
	21	46	i. *e.*
4	1	9	i. O.
	20	2	i. *t.*
	22	19	i. T.
5	1	43	ii. *t.*
	4	31	ii. T.
	16	15	i. *e.*
	19	36	i. O.
6	14	29	i. *t.*
	16	46	i. T.
	17	54	ii. *e.*
	22	49	ii. O.
7	7	55	iii. *t.*
	10	43	i. *e.*
	10	59	iii. T.

May (con.).

d	h	m	
7	14	4	i. O.
8	8	56	i. *t.*
	11	13	i. T.
	14	55	ii. *t.*
	17	43	ii. T.
9	5	12	i. *e.*
	8	31	i. O.
	13	3	iv. t.
	14	59	iv. T.
10	3	23	i. *t.*
	5	40	i. T.
	7	11	ii. *e.*
	12	0	ii. O.
	17	51	iii. *e.*
	20	37	iii. E.
	21	56	iii. *o.*
	23	40	i. *e.*
11	1	0	iii. O.
	2	58	i. O.
	21	49	i. *t.*
12	0	6	i. T.
	4	8	ii. *t.*
	6	56	ii. T.
	18	9	i. *e.*
	21	25	i. O.
13	16	16	i. *t.*
	18	33	i. T.
	20	28	ii. *e.*
14	1	11	ii. O.
	11	30	iii. *t.*
	12	37	i. *e.*
	14	34	iii. T.
	15	52	i. O.
15	10	43	i. *t.*
	13	0	i. T.
	17	20	ii. *t.*
	20	8	ii. T.
16	7	6	i. *e.*
	10	18	i. O.
17	5	10	i. *t.*
	7	27	i. T.
	9	45	ii. *e.*
	13	0	iv. *e.*
	14	15	iv. E.
	14	22	ii. O.
	21	25	iv. *o.*
	21	49	iii. *e.*
	23	25	iv. O.
18	0	36	iii. E.
	1	29	iii. *o.*
	1	34	i. *e.*
	4	33	iii. O.
	4	45	i. O.
	23	36	i. *t.*
19	1	54	i. T.
	6	32	ii. *t.*
	9	20	ii. T.
	20	2	i. *e.*
	23	12	i. O.
20	18	3	i. *t.*
	20	20	i. T.
	23	1	ii. *e.*
21	3	32	ii. O.
	14	31	i. *e.*

May (con.)

21	15	1	iii. *t.*
	17	39	i. O.
	18	5	iii. T.
22	12	30	i. *t.*
	14	47	i. T.
	19	42	ii. *t.*
	22	30	ii. T.
23	8	59	i. *e.*
	12	5	i. O.
24	6	56	i. *t.*
	9	13	i. T.
	12	18	ii. *e.*
	16	42	ii. O.
25	1	47	iii. *e.*
	3	28	i. *e.*
	4	35	iii. E.
	4	57	iii. *o.*
	6	32	i. O.
	8	1	iii. O.
26	1	22	i. *t.*
	3	40	i. T.
	4	36	iv. *t.*
	6	37	iv. T.
	8	53	ii. *t.*
	11	41	ii. T.
	21	56	i. *e.*
27	0	58	i. O.
	19	49	i. *t.*
	22	6	i. T.
28	1	35	ii. *e*
	5	51	ii. O.
	16	25	i. *e.*
	18	28	iii. *t.*
	19	25	i. O.
	21	32	iii. T.
29	14	15	i. *t.*
	16	32	i. T.
	22	2	ii. *t.*
30	0	50	ii. T.
	10	53	i. *e.*
	13	51	i O.
31	8	41	i. *t.*
	10	59	i. T.
	14	53	ii. *e.*
	19	0	ii. O.

June.

1	5	22	i. *e.*
	5	45	iii. *e.*
	8	17	i. O.
	11	26	iii. O.
2	3	8	i. *t.*
	5	25	i. T.
	11	12	ii. *t.*
	14	0	ii. T.
	23	50	i. *e.*
3	2	44	i. O.
	6	51	iv. *e.*
	8	28	iv. E.
	12	30	iv. *o.*
	14	34	iv. O.
	21	34	i. *t.*
	23	51	i. T.
4	4	10	ii. *e.*
	8	8	ii. O.

June (con.).

d	h	m	
4	18	19	i. *e.*
	21	10	i. O.
	21	51	iii. *t.*
5	0	55	iii. T.
	16	0	i. *t.*
	18	17	i. T.
6	0	20	ii. *t.*
	3	9	ii. T.
	12	47	i. *e.*
	15	36	i. O.
7	10	26	i. *t.*
	12	43	i. T.
	17	27	ii. *e.*
	21	16	ii. O.
8	7	16	i. *e.*
	9	43	iii. *e.*
	10	2	i. O.
	14	48	iii. O.
9	4	52	i. *t.*
	7	10	i. T.
	13	29	ii. *t.*
	16	18	ii. T.
10	1	44	i. *e.*
	4	29	i. O.
	23	18	i. *t.*
11	1	36	i. T.
	6	44	ii *e.*
	10	24	ii. O.
	19	24	iv. *t.*
	20	13	i. *e.*
	21	29	iv. T.
	22	55	i. O.
12	1	11	iii. *t.*
	4	15	iii. T.
	17	45	i. *t.*
	20	2	i. T.
13	2	37	ii. *t.*
	5	25	ii. T.
	14	42	i. *e.*
	17	21	i. O.
14	12	11	i. *t.*
	14	28	i. T.
	20	1	ii. *e.*
	23	32	ii. O
15	9	10	i. *e.*
	11	47	i. O.
	13	43	iii. *e.*
	18	7	iii. O.
16	6	37	i. *t.*
	8	54	i. T.
	15	46	ii. *t.*
	18	34	ii. T.
17	3	39	i. *e.*
	6	13	i. O.
18	1	3	i. *t.*
	3	20	i. T.
	9	19	ii. *e.*
	12	39	ii. O.
	22	7	i. *e.*
19	0	39	i. O.
	4	29	iii. *t.*
	7	32	iii. T.
	19	29	i. *t.*
	21	46	i. T.

June (con.).

d	h	m	
10	0	44	iv. *e*.
	2	39	iv. E.
	2	58	iv. *o*.
	4	53	ii. *t*.
	5	4	iv. O.
	7	41	ii. T.
	16	36	i. *e*.
	19	5	i. O.
21	13	55	i. *t*.
	16	12	i. T.
	22	36	ii. *e*.
22	1	46	ii. O.
	11	4	i. *e*.
	13	31	i. O.
	17	41	iii. *e*.
	21	24	iii. O.
23	8	21	i. *t*.
	10	38	i. T.
	18	1	ii. *t*.
	20	49	ii. T.
24	5	33	i. *e*.
	7	57	i. O.
25	2	46	i. *t*.
	5	4	i. T.
	11	54	ii. *e*.
	14	54	ii. O.
26	0	1	i. *e*.
	2	23	i. O.
	7	46	iii. *t*.
	10	49	iii. T.
	21	12	i. *t*.
	23	30	i. T.
27	7	8	ii. *t*.
	9	56	ii. T.
	18	30	i. *e*.
	20	49	i. O.
28	9	47	iv. *t*.
	11	54	iv. T.
	15	38	i. *t*.
	17	55	i. T.
29	1	11	ii. *e*.
	4	1	ii. O.
	12	57	i. *o*.
	15	15	i. O.
	21	37	iii. *o*.
30	0	35	iii. E.
	10	4	i. *t*.
	12	21	i. T.
	20	15	ii. *t*.
	23	3	ii. T.

July.

d	h	m	
1	7	23	i. *o*.
	9	41	i. O.
2	4	30	i. *t*.
	6	47	i. T.
	14	20	ii. *o*.
	17	13	ii. E.
3	1	49	i. *o*.
	4	9	i. E.
	11	2	iii. *t*.
	14	6	iii. T.
	22	56	i. *t*.

July (con.).

d	h	m	
4	1	13	i. T.
	9	22	ii. *t*.
	12	10	ii. T.
	20	15	i. *o*.
	22	37	i. E.
5	17	22	i. *t*.
	19	39	i. T.
6	3	27	ii. *o*.
	6	31	ii. E.
	14	41	i. *o*.
	17	6	i. E.
	17	10	iv. *o*.
	20	50	iv. E.
7	0	54	iii. *o*.
	4	35	iii. E.
	11	48	i. *t*.
	14	5	i. T.
	22	30	ii. *t*.
8	1	18	ii. T.
	9	7	i. *o*.
	11	34	i. E.
9	6	14	i. *t*.
	8	31	i. T.
	16	34	ii. *o*.
	19	49	ii. E.
10	3	33	i. *o*.
	6	3	i. E.
	14	20	iii. *t*.
	17	24	iii. T.
11	0	40	i. *t*.
	2	57	i. T.
	11	37	ii. *t*.
	14	25	ii. T.
	21	59	i. *o*.
12	0	32	i. E.
	19	6	i. *t*.
	21	23	i. T.
13	5	42	ii. *o*.
	9	7	ii. E.
	16	25	i. *o*.
	19	0	i. E.
14	4	11	iii. *o*.
	8	35	iii. E.
	13	32	i. *t*.
	15	49	i. T.
15	0	7	iv. *t*.
	0	45	ii. *t*.
	2	17	iv. T.
	3	33	ii. T.
	10	51	i. *o*.
	13	29	i. E.
16	7	58	i. *t*.
	10	15	i. T.
	18	50	ii. *o*.
	22	25	ii. E.
17	5	18	i. *o*.
	7	58	i. E.
	17	38	iii. *t*.
	20	42	iii. T.
18	2	24	i. *t*.
	4	42	i. T.
	13	53	ii. *t*.
	16	41	ii. T.
	23	44	i. *o*.

July (con.).

d	h	m	
19	2	26	i. E.
	20	51	i. *t*.
	23	8	i. T.
20	7	58	ii. *o*.
	11	43	ii. E.
	18	10	i. *o*.
	20	55	i. E.
21	7	30	iii. *o*.
	12	35	iii. E.
	15	17	i. *t*.
	17	34	i. T.
22	3	1	ii. *t*.
	5	19	ii. T.
	12	36	i. *o*.
	15	24	i. E.
23	7	32	iv. *o*.
	9	43	i. *t*.
	9	44	iv. O.
	12	1	i. T.
	12	38	iv. *e*.
	15	1	iv. E.
	21	6	ii. *o*.
24	1	1	ii. E.
	7	3	i. *o*.
	9	52	i. E.
	20	59	iii. *t*.
25	0	3	iii. T.
	4	10	i. *t*.
	6	27	i. T.
	16	9	ii. *t*.
	18	57	ii. T.
26	1	29	i. *o*.
	4	21	i. E.
	22	36	i. *t*.
27	0	53	i. T.
	10	15	ii. *o*.
	14	20	ii. E.
	19	55	i. *o*.
	22	50	i. E.
28	10	52	iii. *o*.
	16	35	iii. E.
	17	3	i. *t*.
	19	20	i. T.
29	5	18	ii. *t*.
	8	6	ii. T.
	14	22	i. *o*.
	17	18	i. E.
30	11	29	i *t*.
	13	46	i. T.
	23	25	ii. *o*.
31	3	38	ii. E.
	8	48	i. *o*.
	11	47	i. E.
	14	48	iv. *t*.
	17	3	iv. T.

August.

d	h	m	
1	0	23	iii. *t*.
	3	27	iii. T.
	5	56	i. *t*.
	8	13	i. T.
	18	26	ii. *t*.
	21	14	ii. T.

Aug. (con.).

d	h	m	
2	3	15	i. *o*.
	6	16	i. E.
3	0	22	i. *t*.
	2	39	i. T.
	12	35	ii. *o*.
	16	57	ii. E.
	21	42	i. *o*.
4	0	44	i. E.
	14	18	iii. *o*.
	17	22	iii. O.
	17	37	iii. *e*.
	18	49	i. *t*.
	20	36	iii. E.
	21	6	i. T.
5	7	36	ii. *t*.
	10	24	ii. T.
	16	8	i. *o*.
	19	13	i. E.
6	13	16	i. *t*.
	15	33	i. T.
7	1	45	ii. *o*.
	6	15	ii. E.
	10	35	i. *o*.
	13	42	i. E.
8	3	50	iii. *t*.
	6	54	iii. T.
	7	43	i. *t*.
	10	0	i. T.
	20	46	ii. *t*.
	22	29	iv. *o*.
	23	34	ii. T.
9	0	45	iv. O.
	5	2	i. *o*.
	6	37	iv. *e*.
	8	11	i. E.
	9	12	iv. E.
10	2	10	i. *t*.
	4	27	i. T.
	14	56	ii. *o*.
	19	34	ii. E.
	23	29	i. *o*.
11	2	39	i. E.
	17	47	iii. *o*.
	20	37	i. *t*.
	20	52	iii. O.
	21	36	iii. *e*.
	22	54	i. T.
12	0	37	iii. E.
	9	56	ii. *t*.
	12	44	ii. T.
	17	56	i. *o*.
	21	8	i. E.
13	15	4	i. *t*.
	17	21	i. T.
14	4	7	ii. *o*.
	8	53	ii. E.
	12	23	i. *o*.
	15	37	i. E.
15	7	21	iii. *t*.
	9	31	i. *t*.
	10	26	iii. T.
	11	48	i. T.
	23	7	ii. *t*.
16	1	55	ii. T.

Aug. (con.).

d	h	m	
16	6	50	i. *o*.
	10	5	i. E.
17	3	58	i. *t*.
	6	12	iv. *t*.
	6	15	i. T.
	8	32	iv. T.
	17	19	ii. *o*.
	22	12	ii. E.
18	1	17	i. *o*.
	4	34	i. E.
	21	21	iii. *o*.
	22	26	i. *t*.
19	0	25	iii. O.
	0	43	i. T.
	1	36	iii. *e*.
	4	38	iii. E.
	12	19	ii. *t*.
	15	7	ii. T.
	19	44	i. *o*.
	23	3	i. E.
20	16	53	i. *t*.
	19	10	i. T.
21	6	32	ii. *o*.
	11	30	ii. E.
	14	12	i. *o*.
	17	32	i. E.
22	10	57	iii. *t*.
	11	20	i. *t*.
	13	38	i. T.
	14	2	iii. T.
23	1	31	ii. *t*.
	4	19	ii. T.
	8	39	i. *o*.
	12	0	i. E.
24	5	48	i. *t*.
	8	5	i. T.
	19	45	ii. *o*.
25	0	49	ii. E.
	3	7	i. *o*.
	6	29	i. E.
	14	15	iv. *o*.
	16	38	iv. O.
26	0	16	i. *t*.
	0	37	iv. *e*.
	0	58	iii. *o*.
	2	33	i. T.
	3	23	iv. E.
	4	3	iii. O.
	5	35	iii. *e*.
	8	38	iii. E.
	14	43	ii. *t*.
	17	31	ii. T.
	21	34	i. *o*.
27	0	58	i. E.
	18	43	i. *t*.
	21	0	i. T.
28	8	59	ii. *o*.
	14	8	ii. E.
	16	2	i. *o*.
	19	27	i. E.
29	13	11	i. *t*.
	14	39	iii. *t*.
	15	28	i. T.
	17	44	iii. T.

Aug. (con.).

d	h	m	
30	3	56	ii. *t*.
	6	44	ii. T.
	10	29	i. *o*.
	13	55	i. E.
31	7	39	i. *t*.
	9	56	i. T.
	22	13	ii. *o*.

September.

d	h	m	
1	3	27	ii. E.
	4	57	i. *o*.
	8	24	i. E.
2	2	7	i. *t*.
	4	24	i. T.
	4	40	iii. *o*.
	7	46	iii. O.
	9	35	iii. *e*.
	12	39	iii. E.
	17	9	ii. *t*.
	19	58	ii. T.
	22	30	iv. *t*.
	23	25	i. *o*.
3	0	56	iv. T.
	2	53	i. E.
	20	34	i. *t*.
	22	52	i. T.
4	11	28	ii. *o*.
	16	46	ii. E.
	17	53	i. *o*.
	21	22	i. E.
5	15	3	i. *t*.
	17	20	i. T.
	18	24	iii. *t*.
	21	29	iii. T.
6	6	24	ii. *t*.
	9	12	ii. T.
	12	21	i. *o*.
	15	50	i. E.
7	9	31	i. *t*.
	11	48	i. T.
8	0	44	ii. *o*.
	6	5	ii. E.
	6	49	i. *o*.
	10	19	i. E.
9	3	59	i. *t*.
	6	16	i. T.
	8	27	iii. *o*.
	11	33	iii. O.
	13	35	iii. *e*.
	16	40	iii. E.
	19	38	ii. *t*.
	22	27	ii. T.
10	1	17	i. *o*.
	4	48	i. E.
	22	27	i. *t*.
11	0	45	i. T.
	6	58	iv. *o*.
	9	28	iv. O.
	14	0	ii. *o*.
	18	38	iv. *e*.
	19	24	ii. E.
	19	45	i. *o*.
	21	35	iv. E.

Sept. (con.).

d	h	m	
11	23	17	i. E.
12	16	55	i. *t*.
	19	13	i. T.
	22	13	iii. *t*.
13	1	20	iii. T.
	8	54	ii. *t*.
	11	42	ii. T.
	14	13	i. *o*.
	17	45	i. E.
14	11	24	i. *t*.
	13	41	i. T.
15	3	17	ii. *o*.
	8	42	i. *o*.
	8	43	ii. E.
	12	14	i. E.
16	5	52	i. *t*.
	8	10	i. T.
	12	19	iii. *o*.
	15	26	iii. O.
	17	35	iii. *e*.
	20	41	iii. E.
	22	9	ii. *t*.
17	0	58	ii. T.
	3	10	i. *o*.
	6	43	i. E.
18	0	21	i. *t*.
	2	38	i. T.
	16	34	ii. *o*.
	21	38	i. *o*.
	22	2	ii. E.
19	1	12	i. E.
	15	43	iv. *t*.
	18	18	iv. T.
	18	49	i. *t*.
	21	7	i. T.
20	2	7	iii. *t*.
	5	14	iii. T.
	11	26	ii. *t*.
	14	14	ii. T.
	16	7	i. *o*.
	19	41	i. E.
21	13	18	i. *t*.
	15	36	i. T.
22	5	25	ii. *o*.
	10	35	i. *o*.
	11	21	ii. E.
	14	9	i. E.
23	7	47	i. *t*.
	10	4	i. T.
	16	15	iii. *o*.
	19	23	iii. O.
	21	35	iii. *e*.
24	0	42	iii. E.
	0	43	ii. *t*.
	3	31	ii. T.
	5	4	i. *o*.
	8	38	i. E.
25	2	16	i. *t*.
	4	34	i. T.
	19	10	ii. *o*.
	23	32	i. *o*.
26	0	40	ii. E.
	3	7	i. E.
	20	45	i. *t*.

Sept. (con.).

d	h	m	
26	23	2	i. T.
27	6	5	iii. *t*.
	9	13	iii. T.
	14	0	ii. *t*.
	16	49	ii. T.
	18	1	i. *o*.
	21	36	i. E.
28	0	37	iv. *o*.
	3	16	iv. O.
	12	40	iv. *e*.
	15	13	i. *t*.
	15	46	iv. E.
	17	31	i. T.
29	8	29	ii. *o*.
	12	30	i. *o*.
	14	0	ii. E.
	16	4	i. E.
30	9	43	i. *t*.
	12	0	i. T.
	20	16	iii. *o*.
	23	24	iii. O.

October.

d	h	m	
1	1	35	iii. *e*.
	3	18	ii. *t*.
	4	43	iii. E.
	6	7	ii. T.
	6	59	i. *o*.
	10	33	i. E.
2	4	11	i. *t*.
	6	29	i. T.
	21	49	ii. *o*.
3	1	28	i. *o*.
	3	18	ii. E.
	5	2	i. E.
	22	41	i. *t*.
4	0	59	i. T.
	10	8	iii. *t*.
	13	17	iii. T.
	16	37	ii. *t*.
	19	26	ii. T.
	19	57	i. *o*.
	23	30	i. E.
5	17	10	i. *t*.
	19	28	i. T.
6	9	50	iv. *t*.
	11	9	ii. *o*.
	12	34	iv. T.
	14	26	i. *o*.
	16	38	ii. E.
	17	59	i. E.
7	11	39	i. *t*.
	13	57	i. T.
8	0	20	iii. *o*.
	3	29	iii. O.
	5	35	iii. *e*.
	5	55	ii. *t*.
	8	44	iii. E.
	8	44	ii. T.
	8	54	i. *o*.
	12	28	i. E.
9	6	7	i. *t*.
	8	25	i. T.

Oct. (con.).

d	h	m	
10	0	30	ii. *o*.
	3	24	i. *o*.
	5	56	ii. E.
	6	57	i. E.
11	0	38	i. *t*.
	2	55	i. T.
	14	15	iii. *t*.
	17	24	iii. T.
	19	14	ii. *t*.
	21	53	i. *o*.
	22	3	ii. T.
12	1	26	i. E.
	19	7	i. *t*.
	21	25	i. T.
13	13	51	ii. *o*.
	16	23	i. *o*.
	19	16	ii. E.
	19	54	i. E.
14	13	36	i. *t*.
	15	54	i. T.
	19	6	iv. *o*.
	21	56	iv. O.
15	4	27	iii. *o*.
	6	42	iv. *e*.
	7	38	iii. O.
	8	34	ii. *t*.
	9	35	iii. *e*.
	9	57	iv. E.
	10	52	i. *o*.
	11	23	ii. T.
	12	45	iii. E.
	14	23	i. E.
16	8	6	i. *t*.
	10	24	i. T.
17	3	12	ii. *o*.
	5	21	i. *o*.
	8	35	ii. E.
	8	52	i. E.
18	2	36	i. *t*.
	4	53	i. T.
	18	25	iii. *t*.
	21	36	iii. T.
	21	54	ii. *t*.
	23	51	i. *o*.
19	0	43	ii. T.
	3	21	i. E.
	21	5	i. *t*.
	23	23	i. T.
20	16	34	ii. *o*.
	18	20	i. *o*.
	21	49	i. E.
	21	55	ii. E.
21	15	35	i. *t*.
	17	53	i. T.
22	8	38	iii. *o*.
	11	14	ii. *t*.
	11	50	iii. O.
	12	50	i. *o*.
	13	34	iii. *e*.
	14	4	ii. T.
	16	18	i. E.
	16	45	iii. E.
23	4	44	iv. *t*.
	7	38	iv. T.

Oct. (con.).

d	h	m	
23	10	4	i. *t*.
	12	22	i. T.
24	5	56	ii. *o*.
	7	19	i. *o*.
	10	47	i. E.
	11	13	ii. E.
25	4	34	i. *t*.
	6	52	i. T.
	22	38	iii. *t*.
26	0	35	ii. *t*.
	1	49	i. *o*.
	1	51	iii. T.
	3	25	ii. T.
	5	16	i. E.
	23	4	i. *t*.
27	1	22	i. T.
	19	19	ii. *o*.
	20	18	i. *o*.
	23	44	i. E.
28	0	33	ii. E.
	17	34	i. *t*.
	19	52	i. T.
29	12	53	iii. *o*.
	13	57	ii. *t*.
	14	48	i. *o*.
	16	5	iii. O.
	16	47	ii. T.
	17	34	iii. *e*.
	18	13	i. E.
	20	46	iii. E.
30	12	4	i. *t*.
	14	22	i. T.
31	8	42	ii. *o*.
	9	18	i. *o*.
	12	42	i. E.
	13	51	ii. E.
	14	20	iv. *o*.
	17	19	iv. O.

November.

d	h	m	
1	0	45	iv. *e*.
	4	9	iv. E.
	6	34	i. *t*.
	8	52	i. T.
2	2	55	iii. *t*.
	3	18	ii. *t*.
	3	48	i. *o*.
	6	8	iii. T.
	6	9	ii. T.
	7	11	i. E.
3	1	3	i. *t*.
	3	21	i. T.
	22	5	ii. *o*.
	22	17	i. *o*.
4	1	39	i. E.
	3	11	ii. E.
	19	34	i. *t*.
	21	52	i. T.
5	16	40	ii. *t*.
	16	47	i. *o*.
	17	10	iii. *o*.
	19	31	ii. T.
	20	8	i. E.

Nov. (con.).

d	h	m	
5	20	24	iii. O.
	21	34	iii. *e*.
6	0	47	iii. E.
	14	3	i. *t*.
	16	21	i. T.
7	11	17	i. *o*.
	11	29	ii. *o*.
	14	37	i. E.
	16	30	ii. E.
8	8	34	i. *t*.
	10	52	i. T.
9	0	17	iv. *t*.
	3	23	iv. T.
	5	47	i. *o*.
	6	3	ii. *t*.
	7	13	iii. *t*.
	8	53	ii. T.
	9	6	i. E.
	10	28	iii. T.
10	3	4	i. *t*.
	5	22	i. T.
11	0	17	i. *o*.
	0	53	ii. *o*.
	3	34	i. E.
	5	49	ii. E.
	21	34	i. *t*.
	23	52	i. T.
12	18	47	i. *o*.
	19	25	ii. *t*.
	21	30	iii. *o*.
	22	3	i. E.
	22	16	ii. T.
13	0	45	iii. O.
	1	35	iii. *e*.
	4	49	iii. E.
	16	4	i. *t*.
	18	22	i. T.
14	13	17	i. *o*.
	14	17	ii. *o*.
	16	32	i. E.
	19	8	ii. E.
15	10	34	i. *t*.
	12	52	i. T.
16	7	47	i. *o*.
	8	48	ii. *t*.
	11	0	i. E.
	11	34	iii. *t*.
	11	39	ii. T.
	14	50	iii. T.
17	5	4	i. *t*.
	7	22	i. T.
	10	7	iv. *o*.
	13	19	iv. O.
	18	47	iv. *e*.
	22	19	iv. E.
18	2	17	i. *o*.
	3	41	ii. *o*.
	5	29	i. E.
	8	27	ii. E.
	23	35	i. *t*.
19	1	53	i. T.
	20	47	i. *o*.
	22	11	ii. *t*.
	23	58	i. E.

Nov. (con.)

d	h	m	
20	1	2	ii.
	1	52	iii.
	5	8	iii.
	5	35	iii.
	8	49	iii.
	18	5	i. *t*
	20	23	i. T
21	15	17	i. *o*
	17	6	ii.
	18	27	i. E
	21	46	ii. E
22	12	35	i. *t*
	14	53	i. T
23	9	47	i. *o*
	11	35	ii. *t*
	12	55	i. E
	14	26	ii. T
	15	57	iii. *t*
	19	15	iii. T
24	7	5	i. *t*.
	9	23	i. T.
25	4	17	i. *o*.
	6	31	ii. *o*.
	7	24	i. E
	11	5	ii. E
	20	20	iv. *t*
	23	38	iv. T
26	1	35	i. *t*.
	3	54	i. T.
	22	47	i. *o*.
27	0	58	ii. *t*
	1	53	i. E
	3	50	ii. T
	6	15	iii.
	9	33	iii.
	9	35	iii.
	12	50	iii.
	20	6	i. *t*.
	22	24	i. T.
28	17	18	i. *o*.
	19	56	ii. *o*.
	20	21	i. E
29	0	24	ii.
	14	36	i. *t*.
	16	54	i. T.
30	11	48	i. *o*.
	14	22	ii. *t*
	14	50	i. E
	17	14	ii. T
	20	22	iii. *t*
	23	41	iii. T

December.

d	h	m	
1	9	7	i. *t*.
	11	25	i. T.
2	6	18	i. *o*.
	9	19	i. E.
	9	22	ii *o*.
	13	43	ii E
3	3	37	i. *t*.
	5	55	i. T.
4	0	48	i. *o*.
	3	46	ii. *t*

Dec. (con.).				*Dec.* (con.).				*Dec.* (con.).				*Dec.* (con.).				*Dec.* (con.).				*Dec.* (con.).			
h	m		d	h	m		d	h	m		d	h	m		d	h	m		d	h	m		
3	47	i. E.	5	22	47	ii. *o.*	9	8	19	i. *o.*	12	0	9	i. *t.*	14	19	59	ii. *t.*	18	4	51	i. *o.*	
6	20	iv. *o.*	6	3	2	ii. E.		11	13	i. E.		2	28	i. T.		22	52	ii. T.		7	37	i. E.	
6	38	ii. T.		16	38	i. *t.*		12	12	ii. *o.*		16	45	iv. *t.*	15	5	17	iii. *t.*		9	24	ii. *t.*	
9	45	iv. O.		18	56	i. T.		16	21	ii. E.		20	16	iv. T.		8	39	iii. T.		12	17	ii. T.	
10	40	iii. *o.*	7	13	49	i. *o.*	10	5	39	i. *t.*		21	20	i. *o.*		13	10	i. *t.*		19	34	iii. *o.*	
12	51	iv. *e.*		16	45	i. E.		7	57	i. T.	13	0	11	i. E.		15	29	i. T.	19	0	51	iii. E.	
16	30	iv. E.		17	10	ii. *t.*	11	2	50	i. *o.*		1	38	ii. *o.*	16	10	20	i. *o.*		2	11	i. *t.*	
16	50	iii. E.		20	3	ii. T.		5	47	i. E.		5	39	ii. E.		13	8	i. E.		4	30	i. T.	
22	7	i. *t.*	8	0	49	iii. *t.*		6	35	ii. *t.*		18	39	i. *t.*		15	4	ii. *o.*					
0	26	i. T.		4	9	iii. T.		9	27	ii. T.		20	58	i. T.		18	58	ii. E.					
19	19	i. *o.*		11	8	i. *t.*		15	7	iii. *o.*	14	15	50	i. *o.*	17	7	40	i. *t.*					
22	16	i. E.		13	27	i. T.		20	50	iii. E.		18	39	i. E.		9	59	i. T.					

The satellites of Jupiter will be invisible from the beginning of the year until January 10, and from December 20 until the end of the year, Jupiter being too near the Sun.

Satellites I. & II. *disappear* at eclipse on the *p* side from January to June, and *reappear* on the side from July to December.

Satellite III. *disappears* and *reappears* on the *p* side from January to June, on the *f* side from July to December.

There is no eclipse of Satellite IV. in January, February, and March. It *disappears* and *reappears* on the *p* side in April, May, and June, and on the *f* side from July to December.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 19^{h}	Feb. 18^{h}	March. 17^{h}	April. $15\frac{1}{2}^{h}$	May. 14^{h}	June. 13^{h}	July. 11^{h}	Aug. $9\frac{1}{2}^{h}$	Sept. 8^{h}	Oct. $6\frac{1}{2}^{h}$	Nov. $5\frac{1}{2}^{h}$	Dec. 5^{h}	Day.
1		♃4O13	O1234	♃1O24	♃31O4	4O312	42O13	431O2	3O24●	12O34	O1423	423O1	1
2		42O3●	21O43	3O124	32O14	♃41O3	412O3	432O1	1O324	21O34	♃♃O4●	4321O	2
3		431O2	24O31	321O4	1O24●	2O13●	4O312	4312O	24O13	O1234	321O4	♃34O2	3
4		43O12	431O2	32O14	O1234	1O234	♃431O	43O12	412O3	1O324	3O214	♃34O1	4
5		3241O	♃♃43O	O432●	21O34	3O124	342O1	♃41O3	4O132	32O14	31O24	21O43	5
6		2O41●	432O1	41O23	2O134	32O4●	314O2	42O13	♃431O	312O4	2O314	O2134	6
7		1O234	41O32	42O13	3O24●	321O4	O1342	41O23	432O1	34O12	12O34	1O234	7
8		O2134	4O123	41O32	31O24	O124●	2O34●	♃43O2	43O2●	♃41O●	O1423	23O14	8
9		2O34●	421O3	43O12	♃32O1	1O234	21O34	32O14	413O2	♃42O3	14O23	321O4	9
10		31O24	42O31	4312O	413O2	2O143	O3124	321O4	42O13	4O23●	4321O	3O124	10
11	21O43	3O124	314O2	432O1	4O132	1O243	31O24	3O124	12O3●	41O32	43O1●	3O24●	11
12	423O1	321O4	3O124	41O32	421O3	43O12	32O14	1O234	O1324	432O1	431O2	21O43	12
13	431O2	23O14	32O4●	♃4O23	42O13	4321O	31O24	2O134	13O24	3421O	42O31	4O13●	13
14	43O21	1O423	1O24●	2O413	♃4O2●	♃432O	O3142	1O234	32O14	34O12	421O3	41O23	14
15	4231O	4O213	O1234	1O234	431O2	43O12	24O3●	♃O124	31O4●	1O24●	4O123	42O31	15
16	4O213	421O3	12O34	3O124	342O1	41O23	421O3	32O4●	♃3O24	2O134	41O23	4321O	16
17	4O23●	♃43O●	2O134	312O4	31O●●	42O13	4O132	3214O	2O134	O34●●	♃234O	43O12	17
18	421O3	43O12	31O24	32O14	O3142	41O23	431O2	43O12	21O43	1O324	3O14●	43O2●	18
19	♃42O1	4321O	3O124	1O324	12O34	43O12	432O1	41O23	O4123	32O14	31O24	421O3	19
20	31O42	423O1	324O●	O1234	2O134	321O4	431O●	42O13	413O2	321O4	2O314		20
21	3O214	41O23	431O●	2O43●	1O324	32O14	4O12●	41O3●	432O1	3O124	21O34		21
22	231O4	4O213	4O123	1O43●	♃3O24	3O24●	421O3	4O312	431O●	13O24	O1234		22
23	O134●	21O43	412O3	34O12	32O14	1O234	♃2O43	432O●	♃43O2	♃2O13	1O234		23
24	1O234	♃O14●	42O13	4312O	31O4●	2O134	O1324	3421O	42O3●	41O3●	23O14		24
25	♃2O34	3O24●	413O2	432O1	O3142	1O34●	31O24	34O12	421O3	♃4O32	32O4●		25
26	2O134	312O4	43O12	41O2●	142O3	3O124	32O14	1O324	4O123	432O1	341O2		26
27	31O24	23O14	3421O	4O123	42O13	312O4	31O4●	2O134	♃41O2	4321O	423O1		27
28	3O421	1O324	♃34O●	42O3●	41O32	324O1	O124●	1O34●	32O41	43O12	421O3		28
29	3241O		O1432	41O3●	43O12	43O2●	12O34	O1324	312O4	413O2	4O213		29
30	42O31		12O34	43O12	432O1	41O23	2O143	321O4	3O124	42O13	41O23		30
31	41O23		2O134		4312O		O423●	♃32O4		412O3			31

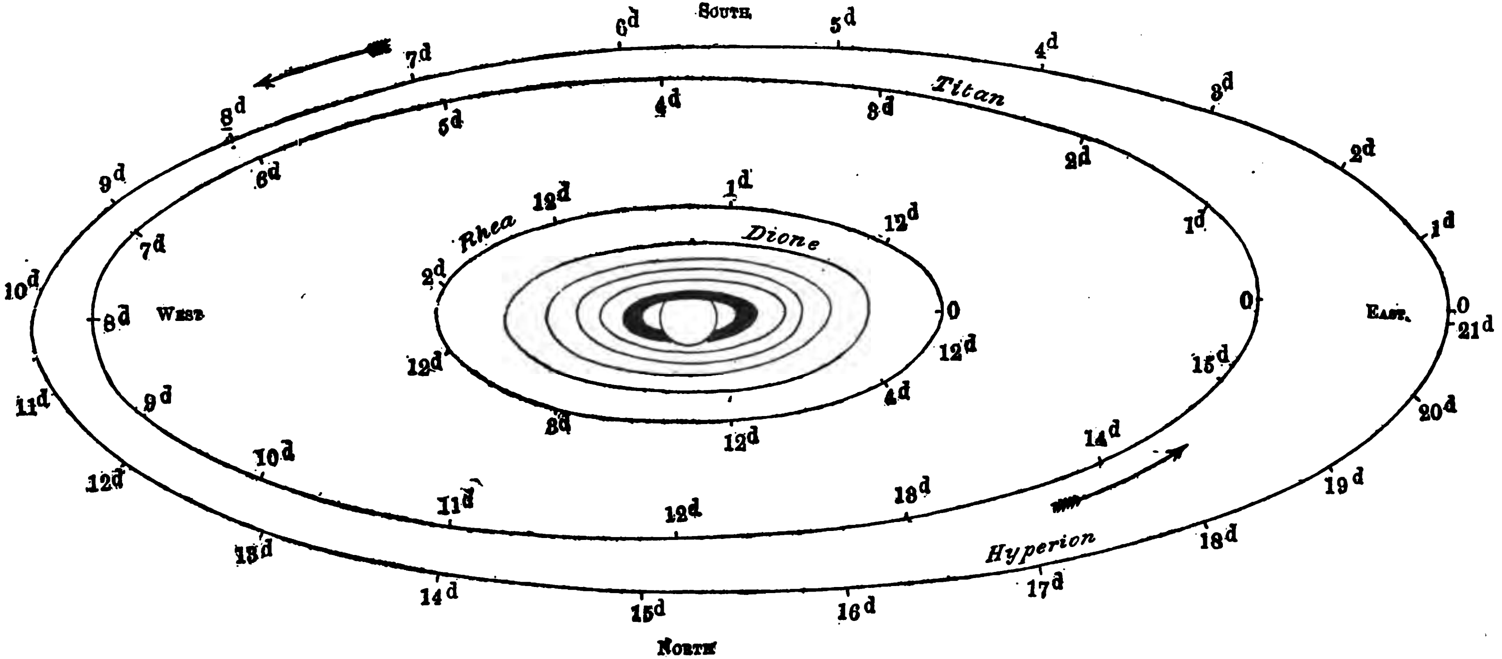
The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

EPHEMERIS OF JUPITER'S SATELLITE V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of $11^{h}\cdot96$ to these.

	d	h		d	h		d	h		d	h
March	20	8·6	May	9	4·2	June	28	23·5	Aug.	17	19·0
	30	7·7		19	3·3	July	8	22·6		27	18·2
April	9	6·9		29	2·4		18	21·7	Sept.	6	17·3
	19	6·0	June	8	1·5		28	20·8		16	16·5
	29	5·1		18	0·5	Aug.	7	19·9		26	15·7
									Oct.	6	14·8

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

Feb.		March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
26	5·3	1	1·2	1	3·6	1	7·4	1	9·7	1	13·3	1	15·6	1	18·0	1	21·8	1	1·8
27	3·9	1	23·8	2	2·2	2	6·0	2	8·3	2	11·9	2	14·2	2	16·6	2	20·4	2	0·4
28	2·5	2	22·4	3	0·9	3	4·6	3	6·9	3	10·5	3	12·9	3	15·2	3	19·1	2	23·0
		3	21·0	3	23·5	4	3·2	4	5·5	4	9·1	4	11·5	4	13·8	4	17·7	3	21·6
		4	19·6	4	22·1	5	1·8	5	4·1	5	7·7	5	10·1	5	12·5	5	16·3	4	20·3
		5	18·3	5	20·7	6	0·5	6	2·7	6	6·4	6	8·7	6	11·1	6	14·9	5	18·9
		6	16·9	6	19·3	6	23·1	7	1·3	7	5·0	7	7·3	7	9·7	7	13·5	6	17·6
		7	15·5	7	18·0	7	21·7	8	0·0	8	3·6	8	5·9	8	8·3	8	12·2	7	16·2
		8	14·1	8	16·6	8	20·3	8	22·6	9	2·2	9	4·5	9	6·9	9	10·8	8	14·8
		9	12·7	9	15·2	9	18·9	9	21·2	10	0·8	10	3·1	10	5·6	10	9·4	9	13·4
		10	11·4	10	13·8	10	17·6	10	19·8	10	23·5	11	1·7	11	4·2	11	8·0	10	12·1
		11	10·0	11	12·4	11	16·2	11	18·4	11	22·1	12	0·3	12	2·8	12	6·6	11	10·7
		12	8·6	12	11·1	12	14·8	12	17·1	12	20·7	12	23·0	13	1·4	13	5·3	12	9·3
		13	7·2	13	9·7	13	13·4	13	15·7	13	19·3	13	21·6	14	0·0	14	3·9	13	7·9
		14	5·8	14	8·3	14	12·0	14	14·3	14	17·9	14	20·2	14	22·7	15	2·5	14	6·5
		15	4·5	15	6·9	15	10·7	15	12·9	15	16·5	15	18·8	15	21·3	16	1·1	15	5·2
		16	3·1	16	5·5	16	9·3	16	11·5	16	15·1	16	17·4	16	19·9	16	23·8	16	3·8
		17	1·7	17	4·2	17	7·9	17	10·1	17	13·7	17	16·1	17	18·5	17	22·4	17	2·4
		18	0·3	18	2·8	18	6·5	18	8·7	18	12·3	18	14·7	18	17·1	18	21·1	18	1·0
		18	22·9	19	1·4	19	5·1	19	7·3	19	10·9	19	13·3	19	15·8	19	19·7	18	23·6
		19	21·6	20	0·0	20	3·7	20	5·9	20	9·6	20	11·9	20	14·4	20	18·3	19	22·3
		20	20·2	20	22·6	21	2·3	21	4·5	21	8·2	21	10·5	21	13·0	21	16·9	20	20·9
		21	18·8	21	21·3	22	0·9	22	3·2	22	6·8	22	9·2	22	11·6	22	15·6	21	19·5
		22	17·4	22	19·9	22	23·5	23	1·8	23	5·4	23	7·8	23	10·2	23	14·2		
		23	16·0	23	18·5	23	22·1	24	0·4	24	4·0	24	6·4	24	8·9	24	12·8		
		24	14·7	24	17·1	24	20·8	24	23·0	25	2·7	25	5·0	25	7·5	25	11·4		
		25	13·3	25	15·7	25	19·4	25	21·6	26	1·3	26	3·6	26	6·1	26	10·0		
		26	11·9	26	14·3	26	18·0	26	20·3	26	23·9	27	2·3	27	4·7	27	8·7		
		27	10·5	27	12·9	27	16·6	27	18·9	27	22·5	28	0·9	28	3·3	28	7·3		
		28	9·1	28	11·5	28	15·2	28	17·5	28	21·1	28	23·5	29	2·0	29	5·9		
		29	7·8	29	10·1	29	13·9	29	16·1	29	19·8	29	22·1	30	0·6	30	4·5		
		30	6·4	30	8·7	30	12·5	30	14·7	30	18·4	30	20·7	30	23·2	31	3·1		
		31	5·0			31	11·1			31	17·0	31	19·4						

ENCELADUS.

Feb.		March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
25	10·4	1	13·0	2	1·5	2	4·9	1	8·3	1	11·6	1	23·7	1	3·2	1	6·7	1	19·3
~~26~~	~~19·3~~	2	21·9	3	10·4	3	13·8	2	17·2	2	20·4	3	8·6	2	12·0	2	15·6	3	4·2
28	4·2	4	6·8	4	19·3	4	22·7	4	2·1	4	5·3	4	17·5	3	20·9	4	0·5	4	13·1
		5	15·7	6	4·2	6	7·6	5	10·9	5	14·2	6	2·3	5	5·8	5	9·4	5	22·0
		7	0·6	7	13·1	7	16·5	6	19·8	6	23·1	7	11·2	6	14·7	6	18·2	7	6·9
		8	9·5	8	22·0	9	1·4	8	4·7	8	8·0	8	20·1	7	23·6	8	3·1	8	15·8
		9	18·4	10	6·9	10	10·3	9	13·6	9	16·8	10	5·0	9	8·4	9	12·0	10	0·7
		11	3·3	11	15·7	11	19·2	10	22·5	11	1·7	11	13·9	10	17·3	10	20·9	11	9·6
		12	12·2	13	0·6	13	4·1	12	7·3	12	10·6	12	22·7	12	2·2	12	5·8	12	18·5
		13	21·1	14	9·5	14	12·9	13	16·2	13	19·5	14	7·6	13	11·1	13	14·7	14	3·4
		15	6·0	15	18·4	15	21·8	15	1·1	15	4·4	15	16·5	14	20·0	14	23·6	15	12·3
		16	14·9	17	3·3	17	6·6	16	10·0	16	13·2	17	1·4	16	4·9	16	8·5	16	21·2
		17	23·8	18	12·1	18	15·5	17	18·9	17	22·1	18	10·3	17	13·8	17	17·4		
		19	8·7	19	21·0	20	0·4	19	3·7	19	7·0	19	19·1	18	22·7	19	2·3		
		20	17·6	21	5·9	21	9·3	20	12·6	20	15·9	21	4·0	20	7·6	20	11·2		
		22	2·4	22	14·8	22	18·1	21	21·5	22	0·7	22	12·9	21	16·5	21	20·1		
		23	11·3	23	23·7	24	3·0	23	6·4	23	9·6	23	21·8	23	1·3	23	5·0		
		24	20·2	25	8·5	25	11·9	24	15·2	24	18·4	25	6·7	24	10·2	24	13·9		
		26	5·1	26	17·4	26	20·8	26	0·1	26	3·3	26	15·6	25	19·1	25	22·8		
		27	14·0	28	2·3	28	5·7	27	8·9	27	12·2	28	0·5	27	4·0	27	7·7		
		28	22·8	29	11·2	29	14·5	28	17·8	28	21·1	29	9·4	28	12·9	28	16·6		
		30	7·7	30	20·1	30	23·4	30	2·7	30	5·9	30	18·3	29	21·8	30	1·5		
		31	16·6							31	14·8					31	10·4		

East Elongations.

TETHYS.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
27 0·0	2 18·7	1 23·8	2 4·8	1 9·5	1 14·1	2 16·0	1 20·8	2 1·8	1 7·0
28 21·4	4 16·0	3 21·1	4 2·1	3 6·8	3 11·4	4 13·3	3 18·1	3 23·2	3 4·4
	6 13·3	5 18·4	5 23·4	5 4·1	5 8·7	6 10·6	5 15·5	5 20·5	5 1·7
	8 10·7	7 15·8	7 20·7	7 1·4	7 6·0	8 7·9	7 12·8	7 17·8	6 23·1
	10 8·0	9 13·1	9 18·0	8 22·7	9 3·3	10 5·2	9 10·1	9 15·1	8 20·4
	12 5·3	11 10·4	11 15·3	10 19·9	11 0·6	12 2·5	11 7·4	11 12·5	10 17·8
	14 2·6	13 7·7	13 12·6	12 17·2	12 21·8	13 23·8	13 4·7	13 9·8	12 15·1
	16 0·0	15 5·0	15 9·9	14 14·5	14 19·1	15 21·1	15 2·0	15 7·1	14 12·4
	17 21·3	17 2·3	17 7·1	16 11·8	16 16·4	17 18·4	16 23·3	17 4·4	
	19 18·6	18 23·6	19 4·4	18 9·1	18 13·7	19 15·7	18 20·7	19 1·8	
	21 15·9	20 20·9	21 1·7	20 6·4	20 11·0	21 13·0	20 18·0	20 23·1	
	23 13·3	22 18·3	22 23·0	22 3·7	22 8·3	23 10·3	22 15·3	22 20·4	
	25 10·6	24 15·6	24 20·3	24 1·0	24 5·6	25 7·6	24 12·6	24 17·7	
	27 7·9	26 12·9	26 17·6	25 22·3	26 2·9	27 4·9	26 9·9	26 15·0	
	29 5·2	28 10·2	28 14·9	27 19·5	28 0·1	29 2·2	28 7·2	28 12·4	
	31 2·5	30 7·5	30 12·2	29 16·8	29 21·4	30 23·5	30 4·5	30 9·7	
					31 18·7				

DIONE.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
26 22·6	1 16·3	3 12·9	3 15·5	2 17·7	2 19·9	1 22·0	1 0·4	1 3·0	2 23·7
	4 10·1	6 6·6	6 9·1	5 11·4	5 13·5	4 15·7	3 18·0	3 20·7	5 17·4
	7 3·8	9 0·3	9 2·8	8 5·0	8 7·2	7 9·3	6 11·7	6 14·4	8 11·1
	9 21·5	11 18·0	11 20·5	10 22·7	11 0·8	10 3·0	9 5·4	9 8·1	11 4·9
	12 15·2	14 11·7	14 14·1	13 16·4	13 18·4	12 20·6	11 23·1	12 1·8	13 22·6
	15 8·9	17 5·4	17 7·8	16 10·0	16 12·0	15 14·3	14 16·8	14 19·6	
	18 2·6	19 23·0	20 1·4	19 3·6	19 5·7	18 8·0	17 10·5	17 13·3	
	20 20·4	22 16·7	22 19·1	21 21·3	21 23·4	21 1·7	20 4·2	20 7·0	
	23 14·1	25 10·4	25 12·8	24 14·9	24 17·0	23 19·3	22 21·9	23 0·7	
	26 7·8	28 4·1	28 6·4	27 8·6	27 10·7	26 13·0	25 15·6	25 18·5	
	29 1·5	30 21·8	31 0·1	30 2·2	30 4·3	29 6·7	28 9·3	28 12·2	
	31 19·2							31 5·9	

RHEA.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
23 2·5	4 3·6	4 19·1	1 21·7	2 12·2	4 2·4	4 16·7	5 7·3	2 10·0	3 1·6
27 15·1	8 16·1	9 7·6	6 10·1	7 0·5	8 14·8	9 5·1	9 19·7	6 22·9	7 14·1
	13 4·6	13 20·0	10 22·4	11 12·9	13 3·1	13 17·4	14 8·2	11 11·0	12 2·7
	17 17·2	18 8·4	15 10·8	16 1·2	17 15·4	18 5·8	18 20·6	15 23·5	16 15·2
	22 5·6	22 20·9	19 23·1	20 13·5	22 3·7	22 18·1	23 9·0	20 12·0	
	26 11·1	27 9·3	24 11·5	25 1·8	26 16·0	27 6·5	27 21·5	25 0·5	
	31 6·6		28 23·8	29 14·1	31 4·4	31 18·9		29 13·0	

TITAN.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
23 4·5	11 4·4	12 3·8	14 0·9	14 20·4	16 15·2	1 12·8	2 8·6	4 6·5	5 6·0
	27 4·2	28 2·4	29 22·7	30 17·8		17 10·4	18 7·3	20 6·0	

East Elongations.

HYPERION.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
d h	d h	d h	d h	d h	d h	d h	d h	d h
13 17·8	3 23·0	16 6·2	6 8·5	18 12·5	8 15·6	20 2·2	11 10·1	1 19·7
	25 3·4		27 10·6		29 20·2			

IAPETUS.

	d h	d h	d h	d h
E. Elongation	Apr. 10 2·4,	June 27 19·0,	Sept. 14 3·4,	
Inf. Conjunction ...	Apr. 29 9·8,	July 16 16·8,	Oct. 3 13·0,	
W. Elongation	Mar. 1 2·2,	May 19 21·6,	Aug. 5 23·8,	Oct. 24 8·9
Sup. Conjunction ...	Mar. 21 19·4,	June 9 1·2,	Aug. 26 3·6,	Nov. 14 1·9

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 5	6 48·4	34·94	15·18	25 44·6 N.	25 47·5 N.
25	6 56·1	35·22	15·07	25 19·6 N.	25 42·6 N.
Feb. 14	7 2·2	35·82	15·08	24 54·3 N.	25 37·6 N.
Mar. 6	7 6·7	36·71	15·24	24 31·8 N.	25 32·4 N.
26	7 9·6	37·83	15·54	24 14·6 N.	25 26·9 N.
April 15	7 11·0	39·11	15·96	24 5·5 N.	25 21·2 N.
May 5	7 11·1	40·40	16·49	24 5·7 N.	25 15·4 N.
25	7 9·8	41·54	17·06	24 15·1 N.	25 9·5 N.
June 14	7 7·4	42·35	17·58	24 31·8 N.	25 3·4 N.
July 4	7 3·9	42·67	17·95	24 52·4 N.	24 57·1 N.
24	7 0·1	42·43	18·08	25 12·7 N.	24 50·7 N.
Aug. 13	6 56·7	41·70	17·94	25 29·2 N.	24 44·0 N.
Sept. 2	6 54·6	40·60	17·58	25 39·5 N.	24 37·1 N.
22	6 54·4	39·32	17·06	25 42·5 N.	24 30·1 N.
Oct. 12	6 56·4	38·04	16·45	25 37·8 N.	24 23·0 N.
Nov. 1	7 0·0	36·89	15·84	25 25·4 N.	24 15·7 N.
21	7 4·7	35·97	15·26	25 5·5 N.	24 8·2 N.
Dec. 11	7 9·8	35·34	14·74	24 38·7 N.	24 0·6 N.
31	7 14·3	35·02	14·31	24 6·6 N.	23 52·8 N.

SATELLITES OF URANUS.

East Elongations.

ARIEL.

March.		April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
11	15·7	3	8·1	1	2·1	2	21·9	3	5·2	2	12·0	1	18·1
14	4·2	5	20·6	3	14·7	5	10·5	5	17·7	5	0·5	4	6·6
16	16·7	8	9·1	6	3·2	7	23·1	8	6·3	7	13·0	6	19·1
19	5·1	10	21·7	8	15·8	10	11·8	10	19·0	10	1·6	9	7·5
21	17·6	13	10·2	11	4·5	13	0·3	13	7·5	12	14·1		
24	6·1	15	22·7	13	17·0	15	12·9	15	20·1	15	2·6		
26	18·6	18	11·3	16	5·6	18	1·6	18	8·7	17	15·1		
29	7·1	20	23·8	18	18·3	20	14·1	20	21·2	20	3·6		
31	19·6	23	12·4	21	6·8	23	2·7	23	9·8	22	16·1		
		26	1·0	23	19·4	25	15·4	25	22·4	25	4·6		
		28	13·5	26	8·1	28	3·9	28	10·9	27	17·1		
				28	20·6	30	16·5	31	23·5	30	5·6		
				31	9·2								

UMBRIEL.

March.		April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
8	16·0	2	12·7	1	14·1	3	20·2	2	22·7	1	0·3	3	4·3
12	19·4	6	16·2	5	17·8	8	0·0	7	2·4	5	3·9	7	7·7
16	22·8	10	19·8	9	21·5	12	3·8	11	6·1	9	7·4	11	11·0
21	2·2	14	23·4	14	1·3	16	7·6	15	9·8	13	11·0	15	14·4
25	5·7	19	3·1	18	5·1	20	11·3	19	13·5	17	14·5	19	17·7
29	9·1	23	6·7	22	8·9	24	15·1	23	17·1	21	18·0	23	21·0
		27	10·4	26	12·6	28	18·9	27	20·7	25	21·5		
				30	16·4					30	0·9		

TITANIA.

March.		April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
12	20·7	7	23·8	4	5·2	8	6·5	4	13·5	8	12·4	3	15·4
21	13·4	16	17·4	12	23·4	17	0·9	13	7·5	17	5·7	12	7·9
30	6·5	25	11·2	21	17·7	25	19·2	22	1·3	25	22·7	21	0·2
				30	12·1			30	19·0			29	16·2
												October.	
												8	8·0

OBERON.

March.		April.		May.		June.		July.		August.	
d	h	d	h	d	h	d	h	d	h	d	h
26	15·8	9	3·7	6	6·3	2	11·3	13	6·1	9	7·6
		22	16·1	19	20·7	16	1·9	26	19·2	22	19·2
						29	16·2				

Apparent Apsides.

	Position Angle.	Apparent Distances.			
		Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
April 7	271·3	14·3	19·9	32·6	43·6
June 6	285·7	14·6	20·4	33·4	44·7
August 5	299·2	14·2	19·8	32·6	43·5

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		September.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
5	4·6	3	14·2	4	23·6	3	1·8	2	10·9	6	17·2	6	2·7
11	1·7	9	11·3	10	20·7	8	22·8	8	7·9	12	14·3	11	23·8
16	22·9	15	8·4	16	17·7	14	19·8	14	5·0	18	11·4	17	21·0
22	20·0	21	5·5			20	16·8	20	2·0	24	8·5	23	18·1
28	17·1	26	2·6			26	13·9	25	23·1	30	5·6	29	15·2
								31	20·1				

Position-angle and distance of the apse:—Feb. 2, 262°·9, 16″·8; Sept. 26, 268°·2, 16″·4; December 15, 266°·9, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1901.	P.	D.	L.	1901.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	− 2 1	−3 13	256 29	July 5	+ 0 54	+3 29	334 42
6	+ 0 26	3 47	190 38	10	− 1 23	4 0	268 32
11	2 51	4 19	124 48	15	3 38	4 29	202 21
16	5 14	4 49	58 57	20	5 51	4 56	136 11
21	7 33	5 17	353 8	25	8 0	5 22	70 3
26	9 47	5 42	287 19	30	10 5	5 45	3 55
31	11 54	6 4	221 28	Aug. 4	12 5	6 6	297 48
Feb. 5	13 56	6 24	155 38	9	13 59	6 24	231 40
10	15 49	6 41	89 48	14	15 47	6 40	165 34
15	17 35	6 54	23 57	19	17 28	6 53	99 30
20	19 12	7 4	318 7	24	19 1	7 3	33 26
25	20 40	7 11	252 17	29	20 27	7 10	327 22
Mar. 2	21 59	7 14	186 26	Sept. 3	21 45	7 14	261 19
7	23 8	7 15	120 33	8	22 54	7 15	195 18
12	24 8	7 12	54 39	13	23 54	7 13	129 17
17	24 57	7 5	348 45	18	24 45	7 7	63 17
22	25 36	6 56	282 50	23	25 26	6 59	357 17
27	26 5	6 43	216 54	28	25 57	6 47	291 18
Apr. 1	26 23	6 27	150 56	Oct. 3	26 19	6 32	225 18
6	26 30	6 9	84 56	8	26 29	6 15	159 20
11	26 26	5 48	18 56	13	26 28	5 54	93 23
16	26 11	5 24	312 56	18	26 17	5 31	27 26
21	25 45	4 58	246 54	23	25 53	5 5	321 29
26	25 8	4 30	180 50	28	25 18	4 37	255 32
May 1	24 20	4 0	114 45	Nov. 2	24 31	4 7	189 36
6	23 21	3 29	48 40	7	23 32	3 35	123 41
11	22 11	2 56	342 33	12	22 22	3 0	57 46
16	20 52	2 22	276 26	17	21 0	2 25	351 51
21	19 22	1 47	210 16	22	19 27	1 49	285 57
26	17 44	1 11	144 6	27	17 43	1 11	220 3
31	15 56	−0 35	77 57	Dec. 2	15 49	+0 33	154 10
June 5	14 2	+0 1	11 47	7	13 46	−0 5	88 18
10	12 0	0 37	305 37	12	11 36	0 44	22 25
15	9 53	1 13	239 25	17	9 20	1 22	316 32
20	7 42	1 48	173 14	22	6 56	1 59	250 39
25	5 28	2 23	107 3	27	4 35	2 36	184 48
30	+ 3 11	+2 56	40 53	32	− 2 8	−3 11	118 57

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 23′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1901·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris ...	0 3 37	−39 46·8	58.	S Camelopardi .	5 30 20	+68 44·7
2.	S Sculptoris ...	0 10 22	−32 35·8	59.	U Aurigæ	5 35 42	+31 59·5
3.	T Andromedæ .	0 17 13	+26 26·8	60.	S Columbæ	5 43 13	−31 43·6
4.	T Cassiopeiæ ...	0 17 52	+55 14·6	61.	R Columbæ......	5 46 42	−29 13·2
5.	S Toucani	0 18 27	−62 13·7	62.	U Orionis	5 49 56	+20 9·4
6.	R Andromedæ .	0 18 48	+38 1·8	63.	η Geminorum...	6 8 54	+22 32·2
7.	S Ceti	0 19 1	− 9 52·6	64.	V Aurigæ	6 16 23	+47 42·7
8.	T Sculptoris ...	0 24 20	−38 27·3	65.	V Monocerotis..	6 17 44	− 2 8·8
9.	U Cassiopeiæ ...	0 40 49	+47 42·9	66.	T Monocerotis...	6 19 52	+ 7 8·4
10.	V Andromedæ .	0 44 39	+35 6·9	67.	W Geminorum .	6 29 17	+15 24·5
11.	X Sculptoris ...	0 44 43	−35 27·5	68.	S Lyncis	6 36 1	+58 0·5
12.	W Cassiopeiæ...	0 49 2	+58 1·6	69.	X Geminorum..	6 40 47	+30 22·6
13.	U Cephei	0 53 29	+81 20·5	70.	S Monocerotis .	6 35 31	+ 9 59·1
14.	U Sculptoris ...	1 6 53	−30 38·5	71.	W Monocerotis .	6 47 33	− 7 1·7
15.	U Andromedæ .	1 9 50	+40 11·7	72.	R Lyncis.........	6 53 8	+55 28·2
16.	S Cassiopeiæ ...	1 12 22	+72 5·4	73.	ζ Geminorum ...	6 58 14	+20 42·9
17.	S Piscium	1 12 24	+ 8 24·6	74.	R Geminorum .	7 1 24	+22 51·4
18.	U Piscium	1 17 44	+12 20·9	75.	V Canis Min. ...	7 1 36	+ 9 1·4
19.	R Sculptoris ...	1 22 25	−33 3·3	76.	R Canis Min. ...	7 3 16	+10 10·8
20.	R Piscium	1 25 32	+ 2 22·3	77.	L^2 Puppis	7 10 31	−44 28·9
21.	X Cassiopeiæ ...	1 49 49	+58 46·4	78.	R Canis Maj. ...	7 14 59	−16 12·5
22.	U Persei	1 53 0	+54 20·5	79.	V Geminorum...	7 17 37	+13 16·9
23.	S Arietis	1 59 19	+12 3·1	80.	U Monocerotis..	7 26 4	− 9 34·2
24.	R Arietis.........	2 10 29	+24 35·8	81.	S Canis Min. ...	7 27 21	+ 8 31·8
25.	o Ceti	2 14 21	− 3 25·5	82.	T Canis Min. ...	7 28 30	+11 57·4
26.	S Persei	2 15 45	+58 8·0	83.	X Puppis	7 28 25	−20 41·8
27.	R Ceti............	2 20 58	− 0 37·5	84.	U Canis Min. ...	7 35 58	+ 8 36·7
28.	S Horologii......	2 22 24	−60 0·9	85.	S Geminorum ..	7 37 6	+23 41·0
29.	R Fornacis	2 24 49	−26 32·2	86.	W Puppis	7 42 41	−41 57·2
30.	U Ceti............	2 28 59	−13 35·0	87.	T Geminorum ..	7 43 22	+23 58·8
31.	R Trianguli ...	2 31 2	+33 50·0	88.	U Geminorum .	7 49 14	+22 15·7
32.	T Arietis.........	2 42 48	+17 5·8	89.	V Puppis	7 55 24	−48 58·6
33.	V Persei	2 43 19	+56 34·3	90.	U Puppis	7 56 11	−12 34·0
34.	R Horologii ...	2 49 44	−50 21·0	91.	Y Puppis	8 8 53	−34 50·5
35.	S Horologii......	2 57 42	−51 2·0	92.	R Cancri.........	8 11 6	+12 1·8
36.	Algol	3 1 43	+40 34·5	93.	V Cancri.........	8 16 5	+17 36·0
37.	U Arietis	3 5 34	+14 25·5	94.	V Carinæ	8 26 43	−59 47·3
38.	X Ceti	3 14 23	− 1 25·8	95.	X Carinæ	8 29 8	−58 53·4
39.	R Persei	3 23 44	+35 19·8	96.	U Cancri.........	8 30 6	+19 14·2
40.	U Eridani	3 46 17	−25 15·4	97.	T Velorum	8 34 28	−47 0·9
41.	T Eridani	3 50 59	−24 19·3	98.	S Cancri	8 38 17	+19 23·4
42.	λ Tauri	3 55 12	+12 12·7	99.	R Mali	8 41 20	−27 50·4
43.	W Tauri	4 22 22	+15 53·3	100.	S Hydræ	8 48 24	+ 3 26·5
44.	R Tauri	4 22 53	+ 9 56·5	101.	T Hydræ.........	8 50 51	− 8 45·8
45.	S Tauri	4 23 47	+ 9 43·6	102.	T Cancri	8 51 1	+20 13·7
46.	T Camelopardi .	4 30 27	+65 56·9	103.	S Mali............	9 0 43	−24 41·6
47.	R Reticuli	4 32 31	−63 14·1	104.	W Cancri	9 4 6	+25 39·1
48.	V Tauri	4 46 18	+17 22·3	105.	W Carinæ	9 19 16	−55 32·2
49.	R Orionis	4 53 38	+ 7 58·9	106.	S Antliæ	9 27 58	−28 11·5
50.	R Leporis	4 55 6	−14 57·3	107.	N Velorum	9 28 13	−56 35·8
51.	T Leporis	5 0 38	−22 2·3	108.	S Velorum	9 29 29	−44 46·2
52.	V Orionis	5 0 50	+ 3 58·0	109.	U Velorum	9 29 30	−45 4·5
53.	S Pictoris	5 8 19	−48 37·7	110.	R Carinæ	9 29 45	−62 21·0
54.	R Aurigæ	5 9 18	+53 28·5	111.	X Hydræ	9 30 47	−14 15·0
55.	T Columbæ......	5 15 40	−33 48·6	112.	R Leo. Min. ...	9 39 39	+34 58·1
56.	W Aurigæ	5 20 13	+36 48·9	113.	Z Hydræ.........	9 40 27	−23 33·8
57.	S Orionis.........	5 24 7	− 4 46·3	114.	R Leonis	9 42 14	+11 53·3

Mean Places of Variable Stars for 1901·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
115.	*l* Carinæ.........	9 42 31	−62 3·1	173.	R Triang. Austr.	15 10 54	−66 8·0
116.	V Leonis	9 54 32	+21 44·1	174.	U Coronæ	15 14 9	+32 0·6
117.	S Carinæ.........	10 6 13	−61 3·9	175.	S Libræ	15 15 42	−20 1·8
118.	Y Carinæ	10 29 27	−57 59·3	176.	S Serpentis......	15 17 1	+14 40·1
119.	R Ursæ Maj. ...	10 37 39	+69 17·7	177.	S Coronæ	15 17 22	+31 43·4
120.	R² Hydræ	10 46 36	−28 6·4	178.	S² Libræ.........	15 18 33	−22 33·5
121.	V Hydræ	10 46 49	−20 43·5	179.	U² Libræ.........	15 27 44	−14 59·5
122.	W Leonis	10 48 25	+14 14·6	180.	X Libræ	15 30 29	−20 50·1
123.	U Carinæ	10 53 46	−59 12·2	181.	W Libræ.........	15 32 15	−15 50·9
124.	S Leonis	11 5 44	+ 6 59·9	182.	S Ursæ Minoris.	15 33 24	+78 58·1
125.	S² Centauri......	11 16 9	−61 19·9	183.	U Libræ	15 36 17	−20 51·7
126.	X Centauri	11 44 15	−44 12·3	184.	Z Libræ	15 40 45	−20 48·8
127.	W Centauri ...	11 50 5	−58 42·1	185.	V Coronæ	15 45 59	+39 52·1
128.	R Comæ	11 59 11	+19 20·2	186.	R Serpentis......	15 46 8	+15 26·1
129.	S Muscæ	12 7 27	−69 36·1	187.	R Libræ	15 48 0	−15 56·5
130.	T Virginis	12 9 32	− 5 29·1	188.	R² Libræ.........	15 50 42	−18 0·8
131.	R Corvi	12 14 30	−18 42·3	189.	S Triang. Austr.	15 52 17	−63 29·6
132.	T Crucis	12 15 57	−61 44·0	190.	U Triang. Austr.	15 58 30	−62 38·5
133.	R Crucis	12 18 12	−61 4·8	191.	Z² Scorpii	15 58 41	−23 49·6
134.	T Canum Ven. .	12 25 18	+32 3·1	192.	X Herculis	15 59 41	+47 30·6
135.	Y Virginis	12 28 46	− 3 52·6	193.	Z Scorpii.........	16 0 11	−21 27·8
136.	T Ursæ Maj. ...	12 31 53	+60 1·9	194.	R Herculis	16 1 46	+18 38·3
137.	R Virginis	12 33 29	+ 7 32·0	195.	X Scorpii	16 2 43	−21 15·6
138.	R Muscæ.........	12 36 2	−68 51·8	196.	W Scorpii	16 5 59	−19 52·8
139.	S Ursæ Maj. ...	12 39 37	+61 38·1	197.	U² Herculis ...	16 6 5	+25 19·6
140.	U² Virginis......	12 42 16	+ 4 41·2	198.	S Normæ	16 10 39	−57 39·3
141.	U Virginis	12 46 4	+ 6 5·5	199.	R Scorpii	16 11 45	−22 42·0
142.	S Crucis	12 48 30	−57 53·6	200.	S Scorpii.........	16 11 46	−22 38·9
143.	W Virginis......	13 20 56	− 2 51·9	201.	W Ophiuchi ...	16 16 5	− 7 27·8
144.	V Virginis	13 22 42	− 2 39·6	202.	V Ophiuchi ...	16 21 13	−12 12·2
145.	R Hydræ	13 24 19	−22 46·1	203.	U Herculis......	16 21 26	+19 6·8
146.	S Virginis	13 27 50	− 6 41·2	204.	Y Scorpii	16 23 53	−19 13·4
147.	Z Centauri	13 34 19	−31 7·8	205.	T Ophiuchi......	16 28 4	−15 55·2
148.	T Centauri	13 36 5	−33 5·8	206.	S Ophiuchi......	16 28 34	−16 57·1
149.	T² Centauri ...	13 42 33	−36 22·1	207.	R Aræ............	16 31 31	−56 47·7
150.	W Hydræ	13 43 27	−27 52·3	208.	W Herculis......	16 31 43	+37 32·5
151.	R Canum Ven. .	13 44 42	+40 2·1	209.	Y Herculis	16 32 3	+ 7 18·5
152.	R² Virginis......	13 59 38	− 8 43·4	210.	R Draconis ...	16 32 24	+66 57·6
153.	Z Virginis	14 5 1	−12 50·1	211.	S Herculis	16 47 23	+15 6·5
154.	S² Hydræ	14 5 52	−28 25·1	212.	S² Scorpii	16 48 27	−44 56·4
155.	R Centauri	14 9 26	−59 27·1	213.	R² Scorpii	16 50 19	−30 25·4
156.	R² Centauri......	14 9 59	−57 23·5	214.	V² Scorpii	16 51 51	−33 27·3
157.	Y Boötis	14 17 25	+20 15·5	215.	V² Herculis......	16 56 47	+31 22·2
158.	X Boötis	14 19 29	+16 46·3	216.	R Ophiuchi......	17 2 5	−15 57·6
159.	S Boötis	14 19 34	+54 15·7	217.	T² Herculis......	17 6 49	+27 10·7
160.	S² Virginis	14 22 19	+ 5 7·4	218.	W² Scorpii	17 8 22	−33 19·1
161.	R Camelopardi .	14 25 1	+84 16·9	219.	U Ophiuchi......	17 11 30	+ 1 19·2
162.	V Centauri	14 25 27	−56 26·9	220.	Z Ophiuchi......	17 14 31	+ 1 37·2
163.	V Boötis	14 25 46	+39 18·4	221.	S² Herculis......	17 17 33	+23 1·1
164.	R Boötis	14 32 50	+27 10·0	222.	U² Scorpii	17 35 10	−43 42·3
165.	V Libræ	14 34 51	−17 13·9	223.	X Sagittarii ...	17 41 19	−27 47·5
166.	S Lupi...........	14 46 46	−46 12·2	224.	Y² Scorpii	17 44 20	−33 40·5
167.	U Boötis	14 49 45	+18 5·9	225.	Y Ophiuchi......	17 47 20	− 6 7·2
168.	δ Libræ	14 55 41	− 8 7·5	226.	Z Herculis	17 53 39	+15 8·4
169.	T Triang. Austr.	15 0 30	−68 20·4	227.	T Draconis	17 54 53	+58 13·6
170.	T² Libræ.........	15 0 50	−18 21·0	228.	W Sagittarii ...	17 58 42	−29 35·2
171.	T Libræ	15 5 5	−19 38·5	229.	T Herculis	18 5 21	+31 0·1
172.	Y Libræ	15 6 27	− 5 38·2	230.	S² Sagittarii ...	18 11 0	−34 8·5

Mean Places of Variable Stars for 1901·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
231.	W Lyræ	18 11 30	+36 38·3	288.	S Delphini	20 38 31	+16 43·9
232.	Y Sagittarii	18 15 34	−18 54·3	289.	Y Aquarii	20 39 12	− 5 11·8
233.	V² Sagittarii	18 21 25	−33 22·6	290.	X Cygni	20 39 32	+35 13·7
234.	*d* Serpentis	18 22 8	+ 0 8·2	291.	T Delphini	20 40 46	+16 2·3
235.	T Serpentis	18 23 59	+ 6 13·9	292.	W Aquarii	20 41 13	− 4 26·7
236.	U Sagittarii	18 26 3	−19 11·8	293.	V Aquarii	20 41 49	+ 2 4·5
237.	X Ophiuchi	18 33 38	+ 8 44·5	294.	U Capricorni	20 42 38	−15 8·9
238.	U Coron. Austr.	18 34 21	−37 55·6	295.	R² Cygni	20 42 39	+44 30·4
239.	R Scuti	18 42 12	− 5 48·7	296.	V Delphini	20 43 16	+18 58·3
240.	β Lyræ	18 46 26	+33 14·8	297.	T Aquarii	20 44 43	− 5 30·8
241.	κ Pavonis	18 46 45	−67 21·5	298.	U Pavonis	20 47 15	−63 5·0
242.	R Lyræ	18 52 19	+43 48·9	299.	T Vulpeculæ	20 47 17	+27 52·5
243.	R Coron. Austr.	18 55 13	−37 5·6	300.	Y Cygni	20 48 2	+34 17·1
244.	R Aquilæ	19 1 36	+ 8 4·8	301.	Z² Cygni	20 48 34	+46 58·9
245.	V Lyræ	19 5 12	+29 30·0	302.	X Delphini	20 50 20	+17 15·9
246.	X² Sagittarii	19 8 46	−18 58·7	303.	R² Capricorni	20 56 27	−27 28·8
247.	S Lyræ	19 9 9	+25 50·1	304.	R Vulpeculæ	20 59 59	+23 25·7
248.	U Draconis	19 9 57	+67 7·0	305.	V Capricorni	21 1 51	−24 19·4
249.	W Aquilæ	19 10 3	− 7 12·9	306.	X Capricorni	21 2 53	−21 46·8
250.	T Sagittarii	19 10 32	−17 8·7	307.	Z Capricorni	21 5 6	−16 35·0
251.	R Sagittarii	19 10 53	−19 28·9	308.	S² Aquarii	21 5 48	− 4 25·8
252.	S Sagittarii	19 13 38	−19 12·3	309.	T Cephei	21 8 14	+68 5·3
253.	Z Sagittarii	19 13 51	−21 6·5	310.	R² Aquarii	21 9 52	− 3 18·4
254.	U Lyræ	19 16 39	+37 41·7	311.	X Pegasi	21 16 17	+14 1·5
255.	T Sagittæ	19 17 17	+17 28·0	312.	T Capricorni	21 16 23	−15 34·6
256.	U Aquilæ	19 23 59	− 7 14·8	313.	S Microscopii	21 20 52	−30 16·7
257.	U Vulpeculæ	19 32 18	+20 6·7	314.	Y Capricorni	21 28 58	−14 24·8
258.	T² Aquilæ	19 33 20	+11 29·0	315.	W Cygni	21 32 18	+44 55·9
259.	R Cygni	19 34 10	+49 58·7	316.	S Cephei	21 36 27	+78 10·7
260.	T Pavonis	19 39 38	−72 0·6	317.	U² Cygni	21 37 18	+53 52·6
261.	T³ Cygni	19 40 51	+29 0·5	318.	R³ Cygni	21 38 49	+43 7·9
262.	T² Cygni	19 40 51	+48 32·0	319.	R Gruis	21 42 10	−47 21·7
263.	S Vulpeculæ	19 44 20	+27 2·4	320.	V Pegasi	21 56 5	+ 5 38·7
264.	X Aquilæ	19 46 34	+ 4 12·7	321.	U Aquarii	21 57 56	−17 6·2
265.	χ Cygni	19 46 46	+32 39·8	322.	S Piscis Austr.	21 58 6	−28 31·7
266.	η Aquilæ	19 47 26	+ 0 45·1	323.	T Pegasi	22 4 4	+12 3·3
267.	R² Sagittarii	19 49 47	−29 26·9	324.	R Piscis Austr.	22 12 22	−30 5·9
268.	S Sagittæ	19 51 31	+16 22·3	325.	X Aquarii	22 13 13	−21 23·6
269.	R² Aquilæ	19 52 28	− 2 9·0	326.	S Gruis	22 19 59	−48 56·5
270.	S² Aquilæ	19 53 46	− 8 9·1	327.	S Lacertæ	22 24 40	+39 48·5
271.	Z Cygni	19 58 39	+49 46·0	328.	δ Cephei	22 25 30	+57 54·5
272.	S Cygni	20 3 25	+57 42·1	329.	W Cephei	22 32 41	+57 55·0
273.	U³ Cygni	20 3 52	+46 0·8	330.	R Lacertæ	22 38 53	+41 51·0
274.	R Capricorni	20 5 45	−14 33·8	331.	S Aquarii	22 51 48	−20 52·3
275.	S Aquilæ	20 7 4	+15 19·6	332.	R Pegasi	23 1 41	+10 0·5
276.	τ² Aquilæ	20 8 6	+12 41·9	333.	V Cassiopeiæ	23 7 25	+59 8·8
277.	W Capricorni	20 8 39	−22 16·8	334.	W Pegasi	23 14 49	+25 43·8
278.	R Sagittæ	20 9 32	+16 25·3	335.	S Pegasi	23 15 32	+ 8 22·7
279.	Z Aquilæ	20 9 55	− 6 27·2	336.	R Aquarii	23 38 42	−15 50·0
280.	R Delphini	20 10 8	+ 8 47·3	337.	Z Aquarii	23 47 8	−16 24·3
281.	T² Sagittarii	20 11 9	−39 25·0	338.	V Cephei	23 51 47	+82 38·4
282.	U Cygni	20 16 32	+47 34·9	339.	V Ceti	23 52 50	− 9 30·7
283.	S³ Cygni	20 29 57	+54 37·8	340.	U Pegasi	23 52 55	+15 24·2
284.	W Delphini	20 33 10	+17 56·1	341.	R Cassiopeiæ	23 53 22	+50 50·2
285.	R Microscopii	20 34 1	−29 8·4	342.	W Ceti	23 57 3	−15 13·6
286.	S Capricorni	20 36 5	−19 24·6	343.	Y Cassiopeiæ	23 58 13	+55 7·7
287.	V Cygni	20 38 7	+47 47·2				

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's name refers to Catalogue on pp. 24–26.)

Note.—The times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

Monocer. 66.

($7^d\ 22\frac{1}{2}^h$.)

an.	May	Sept.
h	d h	d h
20	9 21	21 22
Feb.	June	Oct.
7 20	5 21	18 23
Mar.	July	Nov.
16 20	2 21	14 23
	29 22	
Apr.	Aug.	Dec.
12 20	25 22	11 23

W Geminor. 67.

($2^d\ 15\frac{1}{2}^h$.)

Jan.	Apr.	Oct.
d h	d h	d h
8 17	19 8	6 15
16 11	27 2	14 9
24 5		22 3
31 22	May	29 20
Feb.	4 20	
8 16	12 13	Nov.
16 10		6 14
24 4	Aug.	14 8
	5 17	22 2
Mar.	13 11	29 19
3 22	21 4	
11 15	28 22	Dec.
19 9		7 13
27 3	Sept.	15 7
	5 16	23 1
Apr.	13 10	30 18
3 21	21 3	
11 14	28 21	

ζ Geminor. 73.

($5^d\ 0^h$.)

Jan.	Feb.	Apr.
d h	d h	d h
3 2	22 20	4 11
13 6		14 15
23 9	Mar.	24 19
Feb.	5 0	
2 13	15 4	
12 17	25 8	

ζ Geminor. 73 (*continued*).

May	Sept.	Nov.
d h	d h	d h
4 22	3 19	3 17
15 2	13 22	13 20
25 6	24 2	24 0
Aug.	Oct.	Dec.
4 8	4 6	4 4
14 11	14 10	14 7
24 15	24 13	24 11

W Virginis 143.

($8^d\ 5^h$.)

Jan.	May	Sept.
d h	d h	d h
9 20	10 18	8 15
27 2	28 0	25 22
Feb.	June	Oct.
13 9	14 7	13 4
		30 11
Mar.	July	
2 15	1 13	Nov.
19 22	18 20	16 17
Apr.	Aug.	Dec.
6 4	5 2	4 0
23 11	22 9	21 6

X Sagittar. 223.

($2^d\ 21^h$.)

Feb.	May	Sept.
d h	d h	d h
1 5	17 9	6 14
8 5	24 10	13 14
15 6	31 10	20 15
22 6		27 15
	June	
Mar.	7 10	Oct.
1 6	14 11	
8 7	21 11	4 15
15 7	28 11	11 15
22 7		18 16
29 7	July	25 16
	5 11	
Apr.	12 12	Nov.
5 8	19 12	1 16
12 8	26 12	8 17
19 8		15 17
26 9	Aug.	22 17
	2 13	29 17
May	9 13	
	16 13	Dec.
3 9	23 13	
10 9	30 14	6 18

Y Ophiuchi 225.

($6^d\ 5^h$.)

Jan.	Apr.	Aug.
d h	d h	d h
14 20	27 14	8 7
31 23		25 10
	May	
Feb.	14 17	
18 2	31 20	Sept.
Mar.	June	11 13
7 5	17 22	28 16
24 8		
	July	
Apr.	5 1	Oct.
10 11	22 4	15 19

W Sagittar. 228.

($3^d\ 0^h$.)

Jan.	May	Sept.
d h	d h	d h
31 0	2 4	8 16
	9 18	15 21
	17 8	23 11
Feb.	24 23	
7 15		
15 5	June	
22 19	1 13	
	9 3	Oct.
	16 17	1 1
	24 8	8 15
Mar.		16 6
	July	
2 10	1 22	23 20
10 0	9 12	31 10
17 14	17 2	
25 4	24 17	Nov.
	Aug.	8 0
Apr.	1 7	15 15
1 19	8 21	23 5
9 9	16 11	
16 23	24 2	
24 13	31 16	

Y Sagittar. 232.

($1^d\ 19^h$.)

Jan.	May	Sept.
d h	d h	d h
25 0	3 4	1 10
30 19	8 22	7 4
	14 17	12 23
Feb.	20 12	18 17
	26 6	24 12
5 14		30 6
11 8	June	
17 3	1 1	Oct.
22 21	6 19	6 1
28 16	12 14	11 20
	18 8	17 14
	24 3	23 9
Mar.	29 21	29 3
6 10	July	
12 5	5 16	Nov.
17 23	11 11	3 22
23 18	17 5	9 16
29 13	23 0	15 11
	28 18	21 5
Apr.	Aug.	27 0
4 7	3 13	Dec.
10 2	9 7	
15 20	15 2	2 19
21 15	20 20	
27 9	26 15	

U Sagittar. 236.

($2^d\ 23\frac{1}{2}^h$.)

Jan.	Mar.	Apr.
d h	d h	d h
13 3	1 8	17 13
19 20	8 1	24 7
26 14	14 19	
	21 13	May
Feb.	28 7	1 0
2 8		7 18
9 2	Apr.	14 12
15 20	4 1	21 6
22 14	10 19	28 0

U Sagittar. 236 (*continued*).

June	Aug.	Oct.
d h	d h	d h
3 18	3 11	3 3
10 12	10 4	9 21
17 6	16 22	16 15
23 23	23 16	23 9
30 17	30 10	30 3
July	Sept.	Nov.
7 11	6 4	5 21
14 5	12 22	12 15
20 23	19 16	19 9
27 17	26 10	26 2

β Lyræ 240.

(Max. $3^d\ 2^h$.
Secondary Min.
$6^d\ 11^h$.
Max. $9^d\ 16^h$.)

Jan.	May	Sept.
d h	d h	d h
5 5	1 11	7 15
18 3	14 9	20 13
31 1	27 7	
Feb.	June	Oct.
12 23	9 5	3 11
25 21	22 3	16 9
		29 7
Mar.	July	
10 19	5 1	Nov.
23 17	17 23	11 5
	30 21	24 3
Apr.	Aug.	Dec.
5 15	12 19	7 1
18 13	25 17	19 23

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 256.

(2^d 4½^h.)

Feb.		May		Sept.	
d	h	d	h	d	h
6	10	22	19	5	4
13	11	29	20	12	4
20	11	June		19	5
27	12	5	20	26	5
Mar.		12	21	Oct.	
6	13	19	21	3	6
13	13	26	22	10	6
20	14	July		17	7
27	14	3	22	24	8
Apr.		10	23	31	8
3	15	18	0	Nov.	
10	15	25	0	7	9
17	16	Aug.		14	9
24	17	1	1	21	10
May		8	1	28	10
1	17	15	2		
8	18	22	2		
15	18	29	3		

η AQUILÆ 266.

Feb.		Mar.		Apr.	
d	h	d	h	d	h
5	3	5	20	3	13
12	7	13	0	10	17
19	11	20	4	17	21
26	15	27	8	25	1

η AQUILÆ 266 (*continued*).

May		July		Oct.	
d	h	d	h	d	h
2	5	20	4	7	3
9	10	27	8	14	7
16	14	Aug.		21	11
23	18	3	13	28	15
30	22	10	17	Nov.	
June		17	21	4	20
7	3	25	1	12	0
14	7	Sept.		19	4
21	11	1	5	26	8
28	15	8	10	Dec.	
July		15	14	3	12
5	20	22	18	10	17
13	0	29	22	17	21
				25	1

S SAGITTÆ 268.

(3^d 10^h.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
17	2	8	9	19	7
25	11	16	18	27	16
Feb.		25	3	May	
2	20	Apr.		6	1
11	5	2	12	14	10
19	14	10	22	22	20
28	0			31	5

S SAGITTÆ 268 (*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
8	14	6	6	3	23
16	23	14	16	12	8
25	8	23	1	20	17
July		31	10	29	2
3	18	Sept.		Nov.	
12	3	8	19	6	11
20	12	17	4	14	21
28	21	25	14	23	6
				Dec.	
				1	15
				10	0

X CYGNI 290.

(6^d 19^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
10	5	4	21	12	23
26	14	21	7	29	9
Feb.		June		Oct.	
11	23	6	16	15	18
28	8	23	1	Nov.	
Mar.		July		1	3
16	18	9	10	17	12
Apr.		25	20	Dec.	
2	3	Aug.		3	22
18	12	11	5	20	7
		27	14		

T VULPEC. 299.

(1^d 9½^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	9	3	4	4	9
7	20	7	14	8	19
12	7	12	1	13	6
16	17	16	11	17	16
21	3	20	22	22	3
25	14	25	9	26	13
30	0	29	19		
Feb.		June		Oct.	
3	11	3	5	1	0
7	21	7	16	5	10
12	8	12	2	9	20
16	18	16	13	14	7
21	5	20	23	18	17
25	15	25	10	23	4
		29	20	27	14
Mar.		July		Nov.	
2	2	4	6	1	1
6	12	8	17	5	11
10	22	13	3	9	22
15	9	17	14	14	8
19	19	22	0	18	19
24	6	26	11	23	5
28	16	30	21	27	16
Apr.		Aug.		Dec.	
2	3	4	8	2	2
6	14	8	18	6	12
11	0	13	5	10	23
15	10	17	15	15	11
19	21	22	1	19	20
24	7	26	12	24	6
28	18	30	22	28	17

δ CEPHEI 3

(1^d 15^h.)

Jan.		May		[illegible]	
d	h	d	h	[illegible]	
2	4	5	14	[illegible]	
7	13	10	23	[illegible]	
12	21	16	8	[illegible]	
18	6	21	16	[illegible]	
23	15	27	1	27	
29	0				
Feb.		June		O	
3	9	1	10	2	
8	17	6	19	8	
14	2	12	4	13	
19	11	17	12	18	
24	20	22	21	24	
		28	6	29	
Mar.		July		No	
2	5	3	15	4	
7	13	9	0	9	
12	22	14	8	14	
18	7	19	17	20	
23	16	25	2	25	
29	1	30	11	30	
Apr.		Aug.		De	
3	9	4	20	6	
8	18	10	4	11	
14	3	15	13	16	
19	12	20	22	22	
24	21	26	7	27	17
30	5	31	16		

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.	
°	′	″	°	′	″	°	′	″	°	′	″
0		0·0	56	1	25·4	68	2	21·9	78	4	25·0
10		10·2	58	1	32·1	69	2	29·3	79	4	48·5
20		21·0	60	1	39·7	70	2	37·3	80	5	16·2
30		33·3	61	1	43·8	71	2	46·1	81	5	49·3
35		40·4	62	1	48·2	72	2	55·8	82	6	29·6
40		48·4	63	1	52·8	73	3	6·6	83	7	19·7
45		57·7	64	1	57·8	74	3	18·6	84	8	23·3
50	1	8·7	65	2	3·2	75	3	32·1	85	9	46·5
52	1	13·8	66	2	8·9	76	3	47·4			
54	1	19·3	67	2	15·2	77	4	4·9			

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following Star's name are the Nos. in Catalogue, pp. 24–26.)

U CEPHEI 13*.

$(2^{d}\ 11^{h}\ 49\tfrac{1}{2}^{m}.)$

	d	h	m
Jan.	1	19	52
	6	19	31
	11	19	10
	16	18	49
	21	18	28
	26	18	7
	31	17	47
Feb.	5	17	26
	10	17	5
	15	16	44
	20	16	23
	25	16	2
March.	2	15	41
	7	15	20
	12	14	59
	17	14	39
	22	14	18
	27	13	57
April.	1	13	36
	6	13	15
	11	12	54
	16	12	33
	21	12	12
	26	11	51
May.	1	11	31
	6	11	10
	11	10	49
	16	10	28
	21	10	7
	26	9	46
	31	9	25
June.	5	9	4
	10	8	43
	15	8	22
	20	8	2
	25	7	41
	30	7	20
July.	5	6	59
	10	6	38
	15	6	17
	20	5	56
	25	5	35
	30	5	14
August.	4	4	54
	9	4	33
	14	4	12
	19	3	51
	24	3	30
	29	3	9
Sept.	3	2	48
	8	2	27
	13	2	6
	18	1	46
	23	1	25
	28	1	4
Oct.	3	0	43
	8	0	22
	13	0	1
	17	23	40
	22	23	19
	27	22	58
Nov.	1	22	37
	6	22	17
	11	21	56
	16	21	35
	21	21	14
	26	20	53
Dec.	1	20	32
	6	20	11
	11	19	50
	16	19	29
	21	19	9
	26	18	48
	31	18	27

ALGOL 36*.

$(2^{h}\ 20^{m}\ 49^{s}.)$

	d	h	m
Jan.	1	2	18
	6	19	56
	12	13	34
	18	7	12
	24	0	50
	29	18	28
Feb.	4	12	5
	10	5	43
	15	23	21
	21	16	59
	27	10	37
March.	5	4	15
	10	21	52
	16	15	30
	22	9	8
	28	2	46
April.	2	20	24
	8	14	2
	14	7	39
	20	1	17
	25	18	55
May.	1	12	33
June.	25	0	2
	30	17	40
July.	6	11	18
	12	4	56
	17	22	34
July.	23	16	12
	29	9	50
August.	4	3	27
	9	21	5
	15	14	43
	21	8	21
	27	1	59
Sept.	1	19	37
	7	13	14
	13	6	52
	19	0	30
	24	18	8
	30	11	46
Oct.	6	5	24
	11	23	1
	17	16	39
	23	10	17
	29	3	55
Nov.	3	21	33
	9	15	11
	15	8	49
	21	2	26
	26	20	4
Dec.	2	13	42
	8	7	20
	14	0	58
	19	18	36
	25	12	13
	31	5	51

λ TAURI 42*.

$(3^{d}\ 22^{h}\ 52^{m}.)$

	d	h	m
Jan.	3	2	55
	11	0	39
	18	22	24
	26	20	8
Feb.	3	17	53
	11	15	37
	19	13	21
	27	11	6
March.	7	8	50
	15	6	35
	23	4	19
	31	2	3
April.	7	23	48
	15	21	32
	23	19	17

λ TAURI 42*.

(*continued*).

	d	h	m
July.	3	22	57
	11	20	41
	19	18	25
	27	16	9
August.	4	13	54
	12	11	38
	20	9	23
	28	7	7
Sept.	5	4	51
	13	2	36
	21	0	20
	28	22	5
Oct.	6	19	49
	14	17	33
	22	15	18
	30	13	2
Nov.	7	10	47
	15	8	31
	23	6	15
Dec.	1	4	0
	9	1	44
	16	23	29
	24	21	13

R CANIS MAJ. 78†.

$$\left\{\begin{matrix} 1=1^{d}\ 3^{h}\ 16^{m} \\ 2=2\ \ 6\ \ 31 \end{matrix}\right\}$$

	d	h	m
Jan.	1	23	37
	5	9	25
	8	19	12
	12	4	59
	15	14	47
	19	0	34
	22	10	21
	25	20	8
	29	5	56
Feb.	1	15	43
	5	1	30
	8	11	18
	11	21	5
	15	6	52
	18	16	40
	22	2	27
	25	12	14
	28	22	1
March.	4	7	49
	7	17	36
	11	3	23
	14	13	11
March.	17	22	58
	21	8	45
	24	18	33
	28	4	20
	31	14	7
April.	3	23	54
	7	9	42
	10	19	29
	14	5	16
	17	15	4
	21	0	51
	24	10	38
	27	20	26
May.	1	6	13
	4	16	0
	8	1	47
	11	11	35
	14	21	22
	18	7	9
	21	16	57
	25	2	44
	28	12	31
	31	22	19

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 78 † (*continued*).

d	h	m
Sept.		
2	1	51
5	11	39
8	21	26
12	7	13
15	17	1
19	2	48
22	12	35
25	22	23
29	8	10
Oct.		
2	17	57
6	3	44
9	13	32
12	23	19
16	9	6
19	18	54
23	4	41
26	14	28
30	0	16

d	h	m
Nov.		
2	10	3
5	19	50
9	5	37
12	15	25
16	1	12
19	10	59
22	20	47
26	6	34
29	16	21
Dec.		
3	2	9
6	11	56
9	21	43
13	7	30
16	17	18
20	3	5
23	12	52
26	22	40
30	8	27

S Cancri 98.

d	h	m
Jan.		
4	5	54
13	17	32
23	5	10
Feb.		
1	16	48
11	4	25
20	16	3
March.		
2	3	41
11	15	19
21	2	56
30	14	34
April.		
9	2	12
18	13	50
28	1	27
May.		
7	13	5
17	0	43
26	12	21
June.		
4	23	58

d	h	m
June.		
14	11	36
23	23	14
July.		
3	10	52
12	22	29
22	10	7
31	21	45
Sept.		
7	20	16
17	7	54
26	19	31
Oct.		
6	7	9
15	18	47
25	6	25
Nov.		
3	18	2
13	5	40
22	17	18
Dec.		
2	4	56
11	16	33
21	4	11
30	15	49

δ Libræ 168 *.

($2^d\ 7^h\ 51^m$.)

d	h	m
Jan.		
1	17	5
6	8	48
11	0	30
15	16	13
20	7	56
24	23	39
29	15	21
Feb.		
3	7	4
7	22	47
12	14	30
17	6	12
21	21	55
26	13	38
March.		
3	5	21
7	21	4
12	12	46
17	4	29
21	20	12
26	11	55
31	3	37
April.		
4	19	20
9	11	3
14	2	46
18	18	28
23	10	11
28	1	54
May.		
2	17	37
7	9	19
12	1	2
16	16	45
21	8	28
26	0	10
30	15	53

d	h	m
June.		
4	7	36
8	23	19
13	15	1
18	6	44
22	22	27
27	14	10
July.		
2	5	53
6	21	35
11	13	18
16	5	1
20	20	44
25	12	26
30	4	9
August.		
3	19	52
8	11	35
13	3	17
17	19	0
22	10	43
27	2	26
31	18	8
Sept.		
5	9	51
10	1	34
14	17	17
19	8	59
24	0	42
28	16	25
Dec.		
2	20	24
7	12	6
12	3	49
16	19	32
21	11	15
26	2	57
30	18	40

U Coronæ 174 *.

($3^d\ 10^h\ 51^m$.)

d	h	m
Jan.		
3	4	10
10	1	52
16	23	34
23	21	16
30	18	58

d	h	m
Feb.		
6	16	40
13	14	22
20	12	4
27	9	46

U Coronæ 174 * (*continued*).

d	h	m
March.		
6	7	28
13	5	10
20	2	52
27	0	34
April.		
2	22	16
9	19	58
16	17	40
23	15	22
30	13	4
May.		
7	10	46
14	8	28
21	6	10
28	3	52
June.		
4	1	34
10	23	16
17	20	58
24	18	40
July.		
1	16	22
8	14	4
15	11	46
22	9	28
29	7	10

d	h	m
August.		
5	4	52
12	2	34
19	0	16
25	21	58
Sept.		
1	19	40
8	17	22
15	15	4
22	12	46
29	10	28
Oct.		
6	8	10
13	5	52
20	3	34
27	1	16
Nov.		
2	22	58
9	20	40
16	18	22
23	16	4
30	13	47
Dec.		
7	11	29
14	9	11
21	6	53
28	4	35

U Ophiuchi 219 ‡.

$$\left\{\begin{array}{llll} 1 = 0^d & 20^h & 8^m \\ 2 = 1 & 16 & 15 \\ 3 = 2 & 12 & 23 \end{array}\right\}$$

d	h	m
Jan.		
0	10	40
3	19	10
7	3	41
10	12	12
13	20	43
17	5	14
20	13	45
23	22	16
27	6	47
30	15	18
Feb.		
2	23	49
6	8	20
9	16	51
13	1	22

d	h	m
Feb.		
16	9	53
19	18	24
23	2	55
26	11	26
March.		
1	19	57
5	4	28
8	12	59
11	21	29
15	6	0
18	14	31
21	23	2
25	7	33
28	16	4

* Alternate minima only are given; the others can be readily found by adding the interval under the name of tho star.

† Every third minimum only.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

U Ophiuchi 219 ‡

(*continued*).

	d	h	m
April.	1	0	35
	4	9	6
	7	17	37
	11	2	8
	14	10	39
	17	19	10
	21	3	41
	24	12	12
	27	20	43
May.	1	5	14
	4	13	45
	7	22	16
	11	6	47
	14	15	18
	17	23	49
	21	8	20
	24	16	50
	28	1	21
	31	9	52
June.	3	18	23
	7	2	54
	10	11	25
	13	19	56
	17	4	27
	20	12	58
	23	21	29
	27	6	10
	30	14	31
July.	3	23	2
	7	7	33
	10	16	4
	14	0	35
	17	9	6
	20	17	37
	24	2	8
	27	10	39
	30	19	10
August.	3	3	40
	6	12	11
	9	20	42
	13	5	13
	16	13	44
	19	22	15
	23	6	47
	26	15	17
	29	23	48
Sept.	2	8	19
	5	16	50
	9	1	21
	12	9	52
	15	18	23
	19	2	54
	22	11	25
	25	19	56
	29	4	27
Oct.	2	12	58
	5	21	28
	9	5	59
	12	14	30
	15	23	1
	19	7	32
	22	16	3
	26	0	34
	29	9	5
Nov.	1	17	36
	5	2	7
	8	10	38
	11	19	9
	15	3	40
	18	12	11

Z Herculis 226 ‡.

$$\left\{\begin{array}{l} 1 = 2^d\ \ 1^h\ \ 17^m \\ 2 = 3\ \ 23\ \ 50 \\ 3 = 6\ \ \ 1\ \ \ 7 \end{array}\right\}$$

	d	h	m
Jan.	1	16	40
	9	16	19
	17	15	58
	25	15	38

Z Herculis 226 ‡

(*continued*).

	d	h	m
Feb.	2	15	17
	10	14	56
	18	14	35
	26	14	14
March.	6	13	53
	14	13	32
	22	13	11
	30	12	51
April.	7	12	30
	15	12	9
	23	11	48
May.	1	11	27
	9	11	6
	17	10	45
	25	10	25
June.	2	10	4
	10	9	43
	18	9	22
	26	9	1
July.	4	8	40
	12	8	19
	20	7	58
	28	7	38
August.	5	7	17
	13	6	56
	21	6	35
	29	6	14
Sept.	6	5	53
	14	5	32
	22	5	11
	30	4	51
Oct.	8	4	30
	16	4	9
	24	3	48
Nov.	1	3	27
	9	3	6
	17	2	45
	25	2	25
Dec.	3	2	4
	11	1	43
	19	1	22
	27	1	1

V² Cygni.

$(6^d\ 0^h\ 9^m)$

	d	h	m
Jan.	4	20	42
	16	20	59
	28	21	17
Feb.	9	21	34
	21	21	52
March.	5	22	10
	17	22	27
	29	22	45
April.	10	23	2
	22	23	20
May.	4	23	38
	16	23	55
	29	0	13
June.	10	0	30
	22	0	48

V² Cygni.

(*continued*).

	d	h	m
July.	4	1	6
	16	1	23
	28	1	41
August.	9	1	58
	21	2	16
Sept.	2	2	34
	14	2	51
	26	3	9
Oct.	8	3	26
	20	3	44
Nov.	1	4	2
	13	4	19
	25	4	37
Dec.	7	4	54
	19	5	12
	31	5	30

U³ Cygni 273 *.

$(4^d\ 13^h\ 45^m)$

	d	h	m
Jan.	1	14	1
	10	17	31
	19	21	1
	29	0	31
Feb.	7	4	1
	16	7	31
	25	11	1
March.	6	14	32
	15	18	2
	24	21	32
April.	3	1	2
	12	4	32
	21	8	2
	30	11	32
May.	9	15	2
	18	18	32
	27	22	2
June.	6	1	32
	15	5	2
	24	8	32
July.	3	12	2
	12	15	32
	21	19	3
	30	22	33
Aug.	9	2	3
	18	5	33
	27	9	3
Sept.	5	12	33
	14	16	3
	23	19	33
Oct.	2	23	3
	12	2	33
	21	6	3
	30	9	33
Nov.	8	13	3
	17	16	33
	26	20	3
Dec.	5	23	34
	15	3	4
	24	6	34

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

W DELPHINI 284 †.

$1 = 4^{d}\ 19^{h}\ 21^{m}$
$2 = 9\ \ 14\ \ 42$

	d	h	m
Jan.	1	16	37
	16	2	41
	30	12	44
Feb.	13	22	48
	28	8	52
March.	14	18	55
	29	4	59
April.	12	15	2
	27	1	6
May.	11	11	10
	25	21	13
June.	9	7	16
	23	17	20
July.	8	3	24
	22	13	28
Aug.	5	23	31
	20	9	35
Sept.	3	19	38
	18	5	42
Oct.	2	15	46
	17	1	49
	31	11	53
Nov.	14	21	56
	29	8	0
Dec.	13	18	4
	28	4	7

Y CYGNI 300 ‡.

$1 = 1^{d}\ 17^{h}\ 54^{m}$
$2 = 2\ \ 23\ \ 54$
$3 = 4\ \ 16\ \ 49$

	d	h	m
Jan.	1	18	35
	7	18	24
	13	18	13
	19	18	2
	25	17	52
	31	17	41
Feb.	6	17	30
	12	17	19
	18	17	8
	24	16	57
March.	2	16	46
	8	16	36
	14	16	25
	20	16	14
	26	16	3
April.	1	15	52
	7	15	41
	13	15	31
	19	15	20
	25	15	9
May.	1	14	58
	7	14	47
	13	14	36
	19	14	26
	25	14	15
	31	14	4
June.	6	13	53
	12	13	42
	18	13	31
	24	13	21
	30	13	10
July.	6	12	59
	12	12	48
	18	12	37
	24	12	26
	30	12	15
August.	5	12	5
	11	11	54
	17	11	43
	23	11	32
	29	11	21
Sept.	4	11	10
	10	11	0
	16	10	49
	22	10	38
	28	10	27
Oct.	4	10	16
	10	10	5
	16	9	55
	22	9	44
	28	9	33
Nov.	3	9	22
	9	9	11
	15	9	0
	21	8	50
	27	8	39
Dec.	3	8	28
	9	8	17
	15	8	6
	21	7	55
	27	7	44

S ANTLIÆ 106 §.

	d	h	m
Jan.	1	0	55
	4	6	43
	7	12	31
	10	18	19
	14	0	7
	17	5	55
	20	11	43
	23	17	31
	26	23	19
	30	5	7
Feb.	2	10	55
	5	16	43
	8	22	31
	12	4	19
	15	10	7
	18	15	55
	21	21	43
	25	3	31
	28	9	19
March.	3	15	7
	6	20	55
	10	2	43
	13	8	31
	16	14	19
	19	20	7
	23	1	55
	26	7	43
	29	13	31
April.	1	19	19
	5	1	7
	8	6	55
	11	12	43
	14	18	31
	18	0	19
	21	6	7
	24	11	55
	27	17	43
	30	23	31
May.	4	5	19
	7	11	7
	10	16	55
	13	22	43
	17	4	31
	20	10	19
	23	16	7
	26	21	55
	30	3	43
June.	2	9	31
	5	15	19
	8	21	7
	12	2	55
	15	8	43
	18	14	31
	21	20	19
	25	2	7
	28	7	55
July.	1	13	43
	4	19	31
	8	1	19
	11	7	7
	14	12	55
	17	18	43
	21	0	31
	24	6	19
	27	12	7
Sept.	4	9	43
	7	15	31
	10	21	19
	14	3	7
	17	8	55
	20	14	43
	23	20	31
	27	2	19
	30	8	7
Oct.	3	13	55
	6	19	43
	10	1	31
	13	7	19
	16	13	7
	19	18	55
	23	0	43
	26	6	31
	29	12	19
Nov.	1	18	7
	4	23	55
	8	5	43
	11	11	31
	14	17	19
	17	23	7
	21	4	55
	24	10	43
	27	16	31
	30	22	19
Dec.	4	4	7
	7	9	55
	10	15	43
	13	21	31
	17	3	19
	20	9	7
	23	14	55
	26	20	43
	30	2	31

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† Every third minimum only.

‡ Every fourth minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^{h}\ 46^{m}{\cdot}8$.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 24–26; M signifies maximum; *m*, minimum.

January.

d	No.	
1	27	M
	49	M
	295	M
	288	M
	23	M
	167	*m*
2	38	M
	259	*m*
	325	*m*
	182	*m*
4	178	M
	175	*m*
5	285	*m*
6	286	M
	275	*m*
7	122	M
8	321	M
	90	M
	206	M
9	117	*m*
10	208	M
11	166	M
	8	M
	120	M
12	9	*m*
	199	*m*
	213	*m*
13	115	M
14	222	M
	124	M
	80	*m*
15	4	*m*
16	302	M
	263	M
17	335	*m*
19	298	M
20	141	M
	45	*m*
21	126	*m*
22	12	M
	191	M
24	242	*m*
	216	*m*
25	7	M
	148	*m*
26	5	M
	7	M
	18	*m*
28	59	M
	86	M
29	160	M

February.

d	No.	
1	30	M
	80	M
2	231	M

February (*con.*).

d	No.	
3	159	*m*
	312	*m*
4	339	M
	115	*m*
5	309	M
	237	M
6	44	*m*
	146	*m*
	211	*m*
7	315	M
8	55	M
9	327	M
10	48	M
	39	*m*
11	155	*m*
12	111	M
	322	M
	248	*m*
14	265	*m*
15	229	M
	242	M
	74	*m*
	221	*m*
16	82	M
	278	*m*
17	115	M
18	311	*m*
	244	*m*
19	171	M
21	337	M
22	192	M
23	58	M
	24	*m*
	333	*m*
24	177	M
	179	*m*
26	263	*m*
27	68	M
	252	*m*
28	135	M
	77	*m*

March.

d	No.	
1	80	*m*
	210	*m*
3	203	M
	10	M
	180	
4	278	M
	137	*m*
6	251	M
	151	M
	330	M
	297	*m*
7	32	*m*

March (*con.*).

d	No.	
8	148	M
	93	M
	25	*m*
10	285	M
11	83	*m*
	343	*m*
	242	*m*
	115	*m*
12	50	M
	305	M
	262	M
13	279	M
	114	M
14	313	M
	280	*m*
15	112	*m*
	304	*m*
16	14	M
17	212	M
18	194	M
19	275	M
	80	M
	124	*m*
21	110	M
	57	*m*
22	167	M
	163	*m*
23	255	*m*
24	115	M
25	263	M
	295	*m*
26	192	*m*
27	86	*m*
29	38	*m*
30	264	M
	134	M
	191	*m*
	71	*m*
31	1	M
	45	M
	186	*m*

April.

d	No.	
1	270	M
	202	*m*
2	316	M
3	242	M
	165	M
	185	M
5	117	M
	85	M
	178	*m*
	54	*m*
7	79	M
8	27	*m*
9	119	M

April (*con.*).

d	No.	
10	301	M
	315	*m*
11	164	M
	144	M
12	200	M
	22	M
	155	M
14	175	M
	130	M
16	277	*m*
	115	*m*
	306	*m*
	80	*m*
17	282	*m*
18	139	M
	65	M
	76	*m*
19	64	M
	18	M
22	292	M
	9	M
	136	*m*
	272	*m*
23	258	M
24	331	M
	8	*m*
25	40	M
	303	M
	20	*m*
26	148	*m*
	242	*m*
27	260	M
	52	M
	62	M
	296	M
	320	M
	217	*m*
28	283	M
	278	*m*
29	81	M
	104	M
	115	M
	63	*m*

May.

d	No.	
1	338	*m*
4	80	M
	199	M
5	263	*m*
7	69	M
	152	M
	21	*m*
8	77	M
	231	*m*
	23	*m*
9	19	M
	48	*m*

May (*con.*).

d	No.	
10	113	M
	190	M
	279	*m*
12	137	M
	255	M
	271	*m*
	229	*m*
13	195	M
14	278	M
	72	*m*
16	61	M
	304	M
17	213	M
	39	M
18	153	M
	145	*m*
19	242	M
	141	*m*
20	308	M
21	319	M
	115	*m*
22	180	M
	311	M
	188	M
	258	M
23	3	*m*
	245	*m*
24	285	*m*
25	56	M
	24	M
	192	M
26	30	*m*
27	31	*m*
28	86	M
29	149	M
31	263	M

June.

d	No.	
1	259	M
	275	*m*
	80	*m*
2	297	M
	150	*m*
3	103	M
4	115	M
6	325	M
	183	M
	117	*m*
7	148	M
8	17	M
	221	M
	333	M
	29	M
9	159	M
	11	M
	26	M
	327	*m*

June (*con.*).

d	No.	
10	126	M
	334	M
11	262	*m*
12	176	M
	242	*m*
	287	*m*
13	170	M
14	100	M
	84	M
	216	M
	46	*m*
15	182	M
	295	M
	127	M
	332	*m*
16	74	M
	49	*m*
	2	*m*
17	27	M
	252	M
19	315	M
	80	M
20	208	*m*
21	281	M
	187	M
	210	M
	220	*m*
22	343	M
	294	M
24	167	*m*
25	201	M
	215	M
	227	*m*
26	204	M
	191	M
	44	M
	4	M
	115	*m*
27	163	M
	192	*m*
28	25	M
29	38	M
	6	*m*

July.

d	No.	
2	312	M
	171	*m*
4	242	M
5	274	M
	140	M
7	111	*m*
	278	*m*
8	211	M
	251	*m*
	336	*m*
9	293	M
	115	M

July (*con.*).

d	No.	
9	302	*m*
11	249	M
	248	M
	263	*m*
12	277	M
	181	M
	32	M
	244	M
	135	*m*
13	323	M
	131	M
	146	M
14	267	*m*
15	175	*m*
17	121	M
	58	*m*
	80	*m*
18	77	*m*
	18	*m*
20	237	*m*
21	279	M
	7	*m*
	155	*m*
22	124	M
23	278	M
24	19	*m*
25	86	*m*
26	196	*m*
	148	*m*
27	285	M
	161	M
	137	*m*
29	229	M
30	48	M
	304	*m*
31	128	M

August.

d	No.	
1	324	M
	115	*m*
	12	*m*
2	8	M
3	179	M
	293	M
4	80	M
5	290	M
	139	*m*
6	68	*m*
7	263	M
	136	M
	71	M
11	306	M
	280	M
	309	*m*
	164	*m*
12	275	M
13	178	M
	99	M

August (*con.*).

d	No.	
13	180	*m*
14	115	M
	305	M
	41	M
	165	*m*
15	141	M
	102	*m*
17	93	*m*
19	69	*m*
	315	*m*
20	242	M
	134	*m*
21	231	M
23	151	*m*
24	333	M
	199	*m*
25	317	*m*
26	192	M
	101	M
	76	M
28	24	*m*
29	235	M
	186	M
	79	*m*
	114	*m*
30	47	M
	206	M
31	117	M
	112	M

September.

d	No.	
1	191	*m*
	33	*m*
	80	*m*
4	255	*m*
5	115	*m*
	92	*m*
6	75	M
	148	M
	295	*m*
	59	*m*
7	83	M
9	172	M
	110	*m*
10	39	*m*
	311	*m*
11	233	M
12	4	M
	167	M
13	242	*m*
14	262	M
	55	M
	271	M
15	3	M
16	5	M
	278	*m*
17	279	*m*

September (*con.*).

d	No.	
17	263	*m*
18	115	M
19	272	M
	155	M
	80	M
	22	*m*
	85	*m*
20	15	M
	52	*m*
	161	*m*
	264	*m*
21	20	M
22	27	*m*
	124	*m*
23	31	M
	321	M
	221	*m*
	38	*m*
24	96	M
	193	M
	202	M
25	337	M
	30	M
	77	M
	86	M
	297	*m*
26	16	M
	116	M
27	269	M
	192	*m*
30	304	M
	327	M

October.

d	No.	
1	57	M
2	278	M
3	343	*m*
4	65	*m*
	137	M
	72	M
5	135	M
	242	M
	200	M
	288	M
	215	*m*
6	184	M
	185	*m*
8	217	M
	31	*m*
9	18	M
	285	*m*
10	333	*m*
	115	*m*
11	218	M
	150	M
	205	M
13	254	M

Maxima and Minima of Variable Stars (continued).

October (*con.*).		October (*con.*).		November		November (*con.*).		November (*con.*).		December (*con.*).		December (*con.*).		December (*con.*).		December (*con.*).	
d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.
13	263 M	23	229 *m*	1	269 M	12	178 *m*	26	139 M	1	126 *m*	10	152 M	16	6 M	23	21 M
	159 *m*	24	115 M		180 M	13	8 *m*		192 M		335 *m*	11	278 M	17	63 *m*	24	38
14	87 M		255 M		64 *m*	15	115 *m*		182 *m*	2	282 M	12	311 M		71 *m*		280
15	171 M		119 *m*	2	210 *m*		316 *m*	27	247 M		19 M		285 M	18	144 M	25	40 M
	9 *m*	25	302 M		117 *m*	19	90 M		295 M	3	220 M		141 *m*	19	196 M		262 *m*
16	252 *m*		148 *m*	5	80 M	20	2 M		245 M		80 *m*	13	7 M		137 *m*	26	260 M
17	313 M	26	275 *m*		298 M	21	242 M		224 M		163 *m*		62 *m*	20	308 M	28	44 *m*
18	307 M		48 *m*	8	34 M		164 M	28	191 M	4	332 M		304 *m*		37 M	29	17 *m*
	80 *m*	27	177 *m*	9	246 M		10 M		195 M	5	111 M	14	68 M		263 M		155 *m*
20	154 M	28	315 M		292 *m*	22	86 *m*		279 M		93 M		242 *m*		197 M		315 *m*
	23 M	29	227 M		325 *m*		216 *m*		115 M		77 *m*	15	39 M		115 *m*		192 *m*
21	213 *m*		242 *m*		277 *m*	23	263 *m*	30	251 M	6	46 M		199 M	21	80 M	30	33 M
22	50 *m*	30	250 M	10	60 M	24	145 M				148 M		165 M		14 M	31	330 M
23	208 M		61 *m*	11	310 M		231 *m*	December		7	338 M		120 M		303 M		
	339 M	31	312 *m*		322 M	25	291 M	1	27 M		51 M		167 *m*	22	166 M		
	175 M		203 *m*		342 M		278 *m*		267 M	9	54 M		211 *m*		297 M		

In the above list the dates given for the Stars indicated by the following numbers are doubtful, as the periods are not known with sufficient accuracy:—5, 15, 29, 33, 60, 121, 122, 134, 140, 167, 170, 197, 204, 227, 246, 254, 256, 258, 262, 269, 270, 274, 283, 293, 298, 302, 307, 310, 319, 334, 337.

MAXIMA OF U PEGASI.

Jan.			April.			June.			August.			Oct.			Dec.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
1	3	3	4	19	45	3	18	55	2	18	5	1	17	16	4	10	23
4	20	59	8	13	42	7	12	52	6	12	2	5	11	12	8	4	20
8	14	56	12	7	39	11	6	49	10	5	59	9	5	9	11	22	16
12	8	53	16	1	35	15	0	46	13	23	56	12	23	6	15	16	13
16	2	50	19	19	32	18	18	43	17	17	53	16	17	3	19	10	10
19	20	47	23	13	29	22	12	40	21	11	50	20	11	0	23	4	7
23	14	44	27	7	26	26	6	36	25	5	47	24	4	57	26	22	4
27	8	41				30	0	33	28	23	44	27	22	54	30	16	1
31	2	38										31	16	51			
Feb.			May.			July.			Sept.			Nov.					
			1	1	23												
3	20	35	4	19	20	3	18	30	1	17	40	4	10	48			
7	14	31	8	13	17	7	12	27	5	11	37	8	4	44			
11	8	28	12	7	14	11	6	24	9	5	34	11	22	41			
15	2	25	16	1	11	15	0	21	12	23	31	15	16	38			
18	20	22	19	19	7	18	18	18	16	17	28	19	10	35			
22	14	19	23	13	4	22	12	15	20	11	25	23	4	32			
26	8	16	27	7	1	26	6	12	24	5	22	26	22	29			
			31	0	58	30	0	8	27	23	19	30	16	26			

Every twentieth minimum is given; the others may be found by adding multiples of the period $4^h\ 29^m\ 8^s$.

The minimum takes place $2^h\ 15^m$ after the maximum.

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observer.
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½; 7½	336·7	1·68	8·12	M.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½; 6½	149·6	0·38	9·01	Ls.
3.	β 395	0 32·2	−25 19	6·5; 6·7	286·7	0·51	9·72	Ait.
4.	β 865	0 38·2	+42 40	8 ; 8½	192·0	1·48	8·95	Ls.
5.	β 866	0 39·1	+42 34	9 ; 9	74·8	1·44	9·01	Ls.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½; 7½	218·6	5·27	10·17	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	330·2	0·42	7·88	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	17·1	1·07	8·84	M.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½; 7	354·0	1·66	6·91	M.
10.	β 1000	1 30·3	−30 26	8 ; 13	356·4	1·44	1·84	β.
11.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·6	1·42	10·05	Ls.
12.	*p* Eridani	1 35·7	−56 0	6 ; 6	224·0	7·35	4·96	S.
13.	Σ 205, γ Androm. BC.	1 57·8	+41 51	5½; 6	114·9	0·40	10·17	Ls.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½; 5½	62·2	9·76	10·08	Ls.
15.	Σ 228	2 7·3	+56 59	6½; 7½	94·2	0·53	10·29	Ls.
16.	Σ 305	2 41·8	+18 58	7½; 8	317·0	2·92	10·02	Ls.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½; 6	203·3	1·11	9·16	Ls.
18.	β 525	2 53·5	+21 13	7½; 7½	137·4	0·20	10·10	Ls.
19.	β 530	3 9·1	+22 33	9½; 10	193·3	2·23	8·10	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½; 7	13·8	0·17	10·10	Ls.
21.	Σ 425	3 33·8	+33 49	7½; 8	91·2	2·51	9·11	M.
22.	OΣ 531	4 0·5	+37 48	7 ; 7½	131·0	1·96	9·19	M.
23.	Σ 518, o^2 Eridani	4 10·6	− 7 49	8½; 10½	73·6	2·39	9·11	Ait.
24.	β 883, AB	4 45·6	+10 54	7½; 8	60·2	0·25	10·19	Ls.
25.	" AC	……	……	7½; 10	157·5	18·36	9·09	Ls.
26.	Σ 535	4 17·7	+11 9	7 ; 8	329·3	1·58	8·08	Ait.
27.	Aldebaran	4 30·2	+16 18	1 ; 13½	109·0	31·29	10·12	Ls.
28.	OΣ 98, ι Orionis	5 2·4	+ 8 22	5½; 7	179·1	1·06	8·17	M.
29.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·6	3·33	10·20	M.
30.	Σ 749	5 30·9	+26 53	6½; 7½	173·8	0·82	9·18	Ls.
31.	Σ 932	6 28·5	+14 49	8 ; 8	324·6	1·94	10·10	Ls.
32.	OΣ 149	6 30·0	+27 22	7 ; 9	277·8	0·68	9·11	Ls.
33.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	116·7	1·39	10·31	Ls.
34.	Sirius	6 40·6	−16 34	1 ; 10	150·9	4·64	10·30	See.
35.	Hough 28	6 50·0	+27 8	9½; 9½	255·7	6·45	7·14	Ho.
36.	Σ 1037	7 6·6	+27 24	7 ; 7	302·5	0·77	10·30	Ls.
37.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	109·2	1·50	7·25	M.
38.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	205·3	6·89	6·08	C.
39.	Σ 1074	7 15·4	+ 0 36	7½; 8½	140·4	0·72	9·26	Cog.
40.	OΣ 175	7 27·7	+31 10	6 ; 6½	334·5	0·61	9·15	Ls.
41.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	226·4	5·95	10·19	Ls.
42.	Procyon	7 34·0	+ 5 29	1 ; 8½	338·3	4·83	10·24	Ls.
43.	Σ 1187	8 3·2	+32 31	7 ; 8	224·9	2·17	9·12	Ls.
44.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½; 7½	6·1	1·14	10·34	M.
45.	" " AC	……	……	5½; 6½	114·0	5·68	9·12	Ls.
46.	" " BC	……	……	7½; 6½	127·2	5·78	9·12	Ls.
47.	β 205	8 28·8	−24 16	7 ; 7	236·5	0·75	8·25	Ait.
48.	β 208	8 34·6	−22 20	7 ; 8	76·2	0·45	8·29	Ait.
49.	Σ 1273, ε Hydræ AB	……	……	4 ; 6	267·2	0·20	10·24	Ls.
50.	Σ 1273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	230·0	3·48	10·26	Ls.
51.	Σ 3121	9 11·9	+29 0	7½; 7½	16·2	0·77	9·21	Cog.
52.	OΣ 201	9 18·0	+28 20	7 ; 11	220·1	1·31	10·30	Ls.
53.	Σ 1348	9 19·2	+ 6 50	7½; 7½	322·0	1·72	10·31	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½; 7	111·7	0·78	8·91	Ait.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	309·8	2·20	9·29	Ls.
56.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½; 6	91·2	0·48	8·91	Ait.
57.	OΣ 215, P. x. 23	10 10·9	+18 15	6½; 7	209·2	0·85	9·41	M.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	118·3	3·67	10·24	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	74·0	1·04	6·30	Ls.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	317·4	0·58	7·34	Ls.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Observer.
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	321·1	0·76	9·39	Ls.
62.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	155·5	2·30	10·30	Ls.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	53·2	2·17	10·43	M.
64.	OΣ 235	11 26·7	+61 38	5½ ; 7	109·6	0·68	8·91	Ait.
65.	β 603	11 53·5	+14 50	6½ ; 10	315·6	0·84	10·44	Ls.
66.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½ ; 8	219·8	1·38	9·29	Ls.
67.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	150·2	5·67	8·35	M.
68.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	77·0	1·08	9·34	Ls.
69.	β 112	12 55·2	+19 1	9 ; 9½	294·6	2·12	9·37	Ls.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	341·5	1·76	8·42	M.
71.	β 933	13 30·0	+33 39	8 ; 8½	29·5	2·32	8·37	Ait.
72.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	134·9	1·04	9·57	M.
73.	Σ 1785	13 44·5	+27 29	7½ ; 8	270·5	1·57	8·44	M.
74.	Σ 1788	13 49·7	− 7 34	6½ ; 7½	75·5	2·60	6·38	M.
75.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	179·7	1·45	10·47	M.
76.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	161·6	0·16	8·44	Ls.
77.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	328·4	2·65	10·53	M.
78.	Σ 1879	14 41·3	+10 4	8 ; 8½	139·5	0·46	8·44	Ls.
79.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	202·7	2·69	10·46	M.
80.	β 31	14 47·8	+19 9	8½ ; 9½	196·6	1·38	9·44	Ls.
81.	OΣ 287	14 47·9	+45 20	7 ; 8	316·3	0·85	6·68	M.
82.	β 239	14 52·7	−27 15	5½ ; 6	316·5	0·99	8·28	Ait.
83.	β 350	15 8·2	−27 10	7 ; 8½	160·2	1·15	2·41	L.
84.	Σ 1932, ι Cor. Bor.	15 14·0	+27 13	6½ ; 7	150·8	0·84	10·55	M.
85.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	3·1	0·65	10·55	M.
86.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½ ; 7½	74·7	1·00	9·64	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	178·0	0·90	8·53	Ls.
88.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	117·0	0·43	9·42	Ls.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	142·9	0·84	10·57	M.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	215·3	0·94	6·46	M.
91.	Σ 2032, σ Coronæ	16 11·1	+34 7	6 ; 6½	210·3	4·38	10·72	Ls
92.	Σ 2055, λ Ophiuchi	16 25·8	+ 2 12	4½ ; 5½	53·2	1·30	10·53	M.
93.	Σ 2084, ζ Herculis	16 37·5	+31 47	3 ; 6½	238·9	0·79	10·59	Ls.
94.	Σ 2114	16 56·5	+ 8 36	6½ ; 8	160·1	1·06	9·45	Ls.
95.	Σ 2140, α Herculis	17 10·1	+14 30	2½ ; 6	115·9	4·78	9·57	Ls.
96.	β 629	17 15·0	+32 10	8½ ; 8½	340·4	1·08	9·50	Ls.
97.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	331·5	0·98	9·57	M.
98.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	256·9	1·76	10·54	M.
99.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	244·6	1·45	10·54	M.
100.	OΣ 358	18 31·3	+16 54	6½ ; 7	193·5	1·85	8·62	M.
101.	β 648	18 54·2	+23 39	6 ; 9	222·4	0·96	9·44	Ls.
102.	γ Coronæ Aust.	19 0·0	−37 12	5½ ; 5½	148·5	1·68	8·46	Ait.
103.	β 248	19 13·4	+22 51	6 ; 9	129·6	1·84	6·86	M.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	303·4	1·50	7·53	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	78·5	0·96	8·71	M.
106.	β 151, β Delphini	20 32·7	+14 15	3½ ; 5½	6·5	0·60	9·77	Ls.
107.	Σ 2744	20 57·8	+ 1 9	6½ ; 7½	167·0	1·42	9·83	Ls.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	119·1	1·47	8·83	M.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	121·5	2·39	7·79	M.
110.	Σ 2909, ζ Aquarii	22 23·7	− 0 32	4 ; 4	320·9	3·08	9·86	M.
111.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	222·7	1·15	7·94	M.
112.	Hough 198	23 11·6	+16 12	8 ; 8½	32·0	2·05	4·76	Ho.
113.	β 80	23 13·2	+ 4 53	8 ; 8½	343·1	0·62	8·78	Ait.
114.	β 720	23 29·0	+30 46	6 ; 6	160·9	0·48	9·77	Ls.
115.	Σ 3050	23 54·6	+33 10	6 ; 6½	212·4	2·55	8·91	Ait.

In the column headed "Observer," the following abbreviations are used:—

L. Leavenworth.	M. Maw.	S. Sellors.	Bd. Barnard.
Ls. Lewis.	Ait. Aitken.	C. Collins.	Ho. Hough.
β. Burnham.	Cog. Cogshall.		

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JAN 16

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXIV. JANUARY, 1901. No. 301.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 December 14.

Mr. E. B. KNOBEL, *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and F. W. DYSON, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. Since the last Meeting 76 presents have been received. Only a few of these need be mentioned, but perhaps the following may be of special interest to the Fellows of the Society:—A book by the Rev. J. M. Bacon called 'By Land and Sky,' presented by the publishers; a volume of the Cordoba Observations, being the second part of the Cordoba Durchmusterung, extending from Declination $-42°$ to $-52°$; Messrs. Warner and Swasey have sent us a beautiful book containing plates of instruments made by the firm; Dr. Weinek has sent us a further instalment of slides made from Lick lunar photographs.

Dr. Kinns then proposed that Mr. Seward, Dr. Smart, and Mr. Inwards be the Auditors of the Society's accounts for the year 1900.

This motion, seconded by *Rev. F. Howlett*, was agreed to unanimously.

Mr. Lewis, reading a paper on ζ Herculis, said:—This star was discovered as double by Sir W. Herschel in 1782, when about $1''\cdot0$ separation; in 1795 he noted it as still in the same quadrant but at smaller distance, while in 1802–3 he failed to get satisfactory measures. Afterwards, in 1821–25, Sir John Herschel and South, although making special efforts, failed to see the companion. Concerning the 1802–3 observations, Dr. See states the stars were separated at the time $1''\cdot24$. Since Herschel's time three revolutions have been completed, and these early discrepancies have not been cleared up, neither can a satisfactory orbit be deduced. Dr. Doolittle only this year deduced perhaps the best possible, but it fails to represent present measures as well as might be wished. Indeed, by looking at the diagram on the screen you will see that the dots representing the positions from 1832 to 1863 fall inside those from 1867 to 1900, so that if one set is satisfied the other will not be. [Other diagrams were shown illustrating the two ellipses, their opening out, and their rotation, with a curve showing the varying period as deduced from any point to the next return.] In my paper the Greenwich meridian observations have been discussed, and they show the motion of the principal star to be such that its mass is the same as that of the component, and they suggest the probable cause of the opening out of the ellipse to be the duplicity of one of the two stars.

Mr. Lewis reviewed the subject of the magnitudes and colours, and finally he dealt with the spectroscopic observations. Pointing out the dangerous nature of deducing parallaxes where the *difference* of spectroscopic velocities entered in directly, he still could not disregard the fact that parallaxes deduced from two different sets gave results agreeing within 0″·02. He therefore adopted a parallax of 0″·14, and from this computed the various elements of motion, which, if not quite accurate, certainly indicated the various quantities within reasonable limits, the total motion in space being about 30 miles.

The President. I think that not many Fellows will be able to offer observations upon this paper without consideration, but as Mr. Lewis has impugned the accuracy of spectroscopic observations, I will call upon Mr. Newall to defend the position which has been assailed.

Mr. Newall. So far as I understood Mr. Lewis, he was paying a great compliment to spectroscopists. He found the spectroscopic observations to run in a systematic way, but with so small an amplitude that he was in doubt as to the propriety of using them. He has used them, and finds he under-estimated their accuracy. He was looking for large variation in orbital motion, and is surprised to find so small a difference. Was I right?

Mr. Lewis. I expected a great difference in velocity, but as the parallaxes came out right, and the total amount of change of velocity in the orbit is only about 5 miles, it all turns out correct. As it is going past the perihelion I expected more increase.

Mr. Newall. The velocity in line of sight would be changing, but by a very small quantity. At periastron it would be *nil.*

Mr. Lewis. The total change from this node to the other would not be more than 5 miles, but the two parallaxes coming out so close gave a little confidence in the measures.

Mr. Newall. I have three observations made at Cambridge, and they are: 1897 June 14, −71·4; 1898 May 16, −68·4; and in 1899 April 21, −74·3. Those were three observations taken with the "*Single-prism Bruce Spectroscope*" at the Cambridge Observatory, and I am inclined to think any one of these may be in error by 3 kilometres.

Mr. Lewis. The mean of these three observations, I suppose, would be fairly accurate?

Mr. Newall. Yes, if one may take a mean.

Mr. Lewis. Then they give the same results practically as Prof. Campbell.

Mr. Dyson. I should like to say how much work has been involved in this paper, and how much we are indebted to Mr. Lewis for bringing it forward. Mr. Lewis has discussed a very difficult double star, and has, to begin with, collected all the observations of every kind he could get about it, so that if anyone differs from his conclusions they have all the data collected together. As he explained, the first part of his paper was essentially the micrometer measures giving the relative positions of the principal star and the 6th mag. companion; and then he discussed the meridian observations of the 3rd mag. star, and found its variations were approximately half those indicated by the micrometer observations, and thus obtained the ratio of the masses of the components. In the third place he examined the remaining discordances and framed an hypothesis to explain them. There is one point about the paper which I think is worth investigation, and that is, if the parallax is as big as $0''\cdot14$ it ought to be determinable directly, and I think Mr. Lewis will admit that he felt himself to be on shaky ground in using spectroscopic observations for the determination of that parallax. This paper will serve to draw attention to the importance of getting that out. It is a complete determination of a very complicated system—the distance velocities and everything connected with it. Of course the explanation of the discordance as due to a third companion is a very troublesome matter, because of the great difficulty of the system considered as a mathematical problem, at any rate at present. I believe Prof. Darwin, as the result of six or seven years' work, can only solve particular cases in which he assumed one star to be a very large mass, the second a smaller mass, and the third a smaller mass still. That shows how impossible it would be to get out a mathematical representation of three ordinary bodies such as has been mentioned of masses more or less equal, so that Mr. Lewis would hardly expect to find any very great accordance in the inequality over a period of any length of time.

The President. Mr. Dyson's comments show how valuable this communication is, and I am sure we shall all return our thanks to the author.

Father Sidgreaves read a paper on the connection between sun-spots and magnetic storms. The paper gives the details and results of the study of the magnetograph curves and sun-spot drawings at Stonyhurst College Observatory, in connection with the Greenwich tabulations of sun-spot areas during the 18 years from 1881 to 1898 inclusive. The object of the study was to collect whatever evidence could be found for or against the reality of a connection between the two phenomena. That the two periods ran together there was no doubt; but it was thought that if the connection were not a mere coincidence, there ought to be some manifestation of a connection between individual spots and earth magnetism, and with this in view the study was directed mainly to individual spots. A selection of the more important spots was accordingly made and tabulated along with magnetic disturbances on a plan which enabled the eye to note with ease any coincidence of magnetic storms, either with spot-area or with positions of a spot on the solar surface.

Considerable care was necessary when more than one important spot was present on the Sun's visible disc in order to avoid giving undue weight to any side of a comparison. The rules followed in these cases are given in detail in the paper.

All the tabulations were entered in the form of magnitudes, both of sun-spot areas and magnetic storms: but the magnitudes are real measures, not estimated quantities, the area unit being $\frac{1}{10000}$ of the Sun's visible hemisphere, obtained by dividing the Greenwich number by 100 and throwing out the fractions; and the magnetic storm magnitudes are in minutes of arc of the Declination magnet divided by 10 and without the fraction. It is necessary to state with reference to the latter magnitudes the adopted definition of a magnetic storm. It was thought in the first place necessary for the object of the enquiry to separate completely those magnetic disturbances which might be attributed to outside influences from those which more probably were effects of changes occurring in the Earth itself; and repeated examinations of the magnetic curves led to the conclusion that the line of separation might be drawn at a day-range of 25′ of arc of the Declination magnet; and it was decided to take no account of any disturbances which did not reach this difference between the extreme readings. And to keep the notation in small figures, without losing their real significations, the unit adopted was 10′ of arc, and the lowest storm magnitude was 3, including ranges between 25′ and 35′. A zero therefore in this notation means *no magnetic storm*, not that there were no movements of the magnet.

The first comparisons instituted were with reference to the positions of spots on the visible disc. These are tabulated in the paper for the separate magnetic magnitudes from 3 to 16 for the east and west limbs of the Sun and the central meridian. The table shows nothing of interest for the separate magnetic magnitudes; but summing them up we have, of the possible coincidences, only 18 per cent. on the eastern limb, 19 per cent. on the central meridian, and 25 per cent. on western limb.

The next comparison was made between the state of things before and after a well-defined maximum of a spot-area. It seemed reasonable, on any hypothesis of direct connection between sun-spots and magnetic storms, to expect more frequent effects during the growth of a spot than while it was declining; but the result was found to be the other way. Taking always an equal period before and after a maximum development of a spot, the sums come out with a decided preference for the *after half* time, not only for all the magnetic storms in general but also separately for each magnitude in particular.

It was the preparation of this table that led to the final results given in Tables III. and IV. of the paper. In Table III. the sun-spots are grouped according to well-defined maximum magnitudes, included between a rise and fall of the numbers expressing their areas; and opposite each group is written the mean sum of the magnetic storm magnitudes found within an adopted period about the spot maximum of the group. This adopted period was in all cases the days about a spot maximum on which its magnitude was not less than half the maximum. Denoting the spot max. by S and the sum of the magnetic storm magnitudes by ΣM, the result appears as follows:—

S	4–6	7 & 8	9 & 10	11 & 12	13–17	over 17
Mean ΣM..	2·6	3·0	4·4	5·4	7·7	16·3

And conversely in Table IV. the magnetic storms are entered in order of magnitudes and opposite each the mean coincident magnitude of the total spotted area of the Sun's visible disc. And these numbers show the same progressive increase of spotted area with increasing magnetic storm magnitudes.

In both tables the extremes are given along with the means; and these show that great magnetic storms have occurred with only small spots, and that large spots have passed through their period of half maximum area without any magnetic storm.

The paper concludes with theoretical inferences, and claims that these wide departures from the close relations indicated by the columns of means show that the real connection between the two phenomena cannot be a direct connection of cause and effect, but must be some common cause of both, which may influence sometimes one, sometimes the other, and often produce the two effects together. This common cause must therefore be something capable of producing darkness in parts of the solar atmosphere, and also of deflecting our magnetic needles; and it must be something moving in space passing nearer to the Earth or nearer to the Sun, or extensive enough to embrace both and produce the two effects together; and finally it must be invisible. It is suggested that all these conditions would be found in aggregations of those minute molecular fragments of Professor J. J. Thomson's recent experiments, and named by him corpuscles. These are highly electrified invisibilities too small to reflect light-waves. Their electrostatic action when near the Sun would darken the vapours thrown up by solar eruptions and at their high velocities when passing near the Earth they would affect our magnetic needles in the same way as the kathode rays are deflected by a magnet in a vacuum-tube.

Prof. Schuster. As Father Sidgreaves says, I have had an opportunity of reading his paper, and I was very much interested in studying the tables which he has shown you, and on which I may make a remark presently. As regards the suggestions made at the end, over which he has rather hurried, I think the Fellows of the Society will find, on reading them carefully, that they are well worthy of consideration. The reason why I encouraged him to publish this portion of the paper was this: we are in the presence of the very mysterious fact of an undoubted connection between sun-spots and terrestrial magnetism and general cosmic physics. Whatever explanation you may offer on that it is bound to bring in some hypothesis which may, at first sight, appear to be wild. Now, I do not believe it is any use running away from a difficulty, and to put aside any hypothesis which may seem wild as unworthy of consideration. I believe, myself, that we should know more about sun-spots if people had been less afraid to theorize about them and hypothesize about them. After all, the best way to kill an hypothesis is to believe in it, and to push it to its logical conclusions, when it will soon appear if it is contradicted by the facts. I had a further interest in the paper because the main suggestion, or one of the suggestions, which it offers is one which I myself made, and on which I have been working for a good many years, and which I have also published, and it is a conclusion from which, to my mind, it is very difficult to escape. If you will allow me to work the matter backwards, I should reason thus: you have on the Earth a daily variation of the magnetic needle, which is greater at sun-spot maximum than at sun-spot minimum. It is more regular than some of the irregular outbursts we have in magnetic storms, and its origin therefore is more likely to be traced. Now, what possible reason can there be for the daily variation being greater on one day than on another? There is no doubt that the daily variation of terrestrial magnetism is due to electric currents which circulate in the upper atmosphere. Now, what difference can there be between a time of sun-spot maximnm and a time of sun-spot minimum? It is a question that I raised, I think, at the British Association Meeting at Manchester, and I repeated it in my Presidential Address at Edinburgh, and the explanation I suggested is that the outer regions of our atmosphere are better conductors of electricity during the time of sun-spot maximum than during the time of sun-spot minimum. It is simply an assumption I make; and if you come to think of it, it is one, I think, that fits in most easily with the facts—for instance, with the well-known periodicity of the Aurora Borealis. Now go one step further. Why should the outer regions of our atmosphere be a better conductor at one time than at another? That leads us very quickly to that very hypothesis which Father Sidgreaves brought before us; and that is, that the whole of space between the Sun and Earth is a better conductor at times of maximum sun-spots than at times of minimum sun-spots. I think, without going into detail, it is matter which, if you once accept it, has the possibility of leading to an explanation. The first periodicity of sun-spots is probably due to outside causes, as, for instance, meteor-streams. You can more easily suppose that than any other cause of periodicity. As regards the actual connection between sun-spots and terrestrial magnetism, I do not know that we have anything very definite, at present, to go upon; also as to the reason why space should be more conductive. Father Sidgreaves offers an explanation by assuming the presence of corpuscles. I do not think we need go so far as that. They are rather fickle. Therefore I think we had better not go beyond the fact that space seems to be a better conductor at the times of sun-spot maximum. There may be other reasons, meteoric streams bringing conducting matter with it, without having to take refuge in the presence of these corpuscles. As to the connection between the Sun and the Earth, I think there can be no doubt whatever that Lord Kelvin's calculations show that it is not electro-magnetic waves that are sent out from the Sun. There can be no doubt whatever that that is absolutely conclusive, but I think his remarks have been misunderstood to be more than they seem to imply. He simply said. if the magnetic storms of the Earth were due to magnetic disturbances sent out from the Sun, the Sun would have to work, in order to procure such disturbances, at so many million horse-power that it is impossible to suppose that would be the case. There may be an action, and it may be a direct action, for if you can imagine a direct discharge of electricity from the Sun reaching our atmosphere and causing electric currents—if you can imagine that you may call it a direct action of the Sun. And such an action is not one which has been disproved by Lord Kelvin's calculations. Coming to the more actual facts disclosed by the tables, they seem to me to indicate one important matter. First of all, I quite agree with the author of the paper that they seem to show a connection between the sun-spots and these magnetic storms; but he has pointed out that sometimes you have large sun-spots without any effect, and some spots of smaller importance which produce electric storms. This points to the fact that two conditions must coincide to produce a magnetic storm. The one condition is this sun-spot; but you must have something else which may be absent or present. If it is absent, you have a large sun-spot without any effect; if it is present, even a small sun-spot may produce a considerable effect. I think that is one of the conclusions which the result of the work seems to point to. At any rate, I think you will agree that it is an investigation which has led to very interesting and important results.

The Astronomer Royal. After the very interesting remarks which have been made by Prof. Schuster I wish to express my sense of the great value of the discussion which Father Sidgreaves has raised as to the connection between individual sun-spots and magnetic storms. It is a matter, I have felt for a long time, upon which we wanted to have a thorough investigation. In cases of very large sun-spots which have been accompanied by magnetic disturbances of a remarkable extent, I have always had a sort of feeling that, as Father Sidgreaves has pointed out, we might be misled by accidental coincidences; and though it is difficult to get over the evidence of a connection, the evidence did not seem to be sufficient, and I am glad to find that Father Sidgreaves has taken the question up on a much larger scale and has discussed it generally. I may say that the way in which he has discussed it seems to me to be a very excellent one, and he has taken account, I am pleased to see, of what may be happening on the other side of the Sun as well as of what is happening on the side turned towards us.

Father Sidgreaves. I think there were no questions proposed which call for answers; but I may be allowed to supply an omission in the epitome I have just given of my paper. It refers to a suggestion made by Professor Tacchini some years ago: that magnetic storms might be found to correspond with marked *changes* occurring on the solar surface. We have not complete data for a thorough investigation of this nature. Faculæ would enter largely into the discussion; and for these we should need a very complete series of photoheliograph records, such as was commenced by Professor Hale at Kenwood. I am indebted to Mr. Townsend, of Sevenoaks, for the means of instituting an indirect comparison of these changes on the Sun's surface with magnetic disturbances. This indirect method is based upon the supposition that such changes would be manifestations of increased solar activity, and that these would be likely to affect the hydrogen lines by reversals or distortions. Mr. Townsend's observations of the red hydrogen line, in the years 1892–6 inclusive, give 231 dates of solar activity represented in this way, and of these 41 dates are noted as strong effects. Comparing these dates with the tabulated magnetic storms, only 53 out of the whole number, and 8 out of the 41 stronger effects, were found to agree with dates of magnetic storms.

The Astronomer Royal then read a paper on observations of Capella as a double star with the 28-inch equatorial of the Royal Observatory at Greenwich. He said that Fellows would remember that, acting on a suggestion made by Mr. Newall as a result of his spectroscopic observations in which he announced the discovery of Capella as a binary star, Mr. Dyson and Mr. Lewis examined the star last April, and discovered that it was visually double. The observations were continued, and the first results were published at the June meeting of the Society. These observations have been continued since at every available opportunity, and Mr. Bryant in particular has watched for Capella at every chance, especially in the daytime, and a considerable number of observations have been accumulated, the double star having been watched through 2½ revolutions, the period of a revolution being 104 days. [Screen.] Naturally in this case they had to depend practically upon position-angles, the estimates of distance being, of course, rather difficult to make. On the diagram three circles had been drawn, on which the observed position-angles had been marked corresponding to the three revolutions which had been under observation. There was an indication from the visual observations that the period might be 103 days, but there might be personality in the observations which might affect that. In the first revolution Capella was observed in the evening twilight, afterwards in the daytime, and latterly east of the meridian in the evening. The first thing which appeared to result from these observations was that the apparent orbit was not very far from circular. There appeared to be a small eccentricity of about 0·05, and the inclination came out as 30° to 35°, the line of nodes agreeing well with the spectroscopic determination of maximum velocity in the line of sight. [Screen.] For the last four observations of Capella κ Pegasi, which was now closing in, was observed on the same nights by the same observer, and it was found that the elongation of Capella was as distinct as of κ Pegasi. In Mr. Lewis's last observation of Capella on Dec. 10, although he was not able to get a satisfactory measure of the position-angle, he satisfied himself of the elongation, and the definition was so fine that he was able to *count* eight complete rings round the star, there being ten or more rings visible. This might serve to indicate that the definition at that time was exceptionally good. These observations were made under great difficulties during fits of fine definition which were usually very short, and it was greatly to the credit of the observers that they had watched so patiently for those fits of fine definition, and that they had managed to secure observations of Capella on some 40 days.

The President. It is impossible to invite discussion at this late hour, but we desire to congratulate the Astronomer Royal upon this triumph of visual observation which he has announced. Certainly it is a remarkable thing as a performance, not only of the instrument, but of the observers, that they should be able to follow the orbit of this double star, discovered by Mr. Newall, and agree with the observations previously made.

The following papers were announced and partly read :—

Rev. W. Sidgreaves. "On the Connection between Solar Spots and Earth Magnetic Storms."

F. W. Henkel. "Watch for the Leonids, 1900, at Markree."

E. E. Barnard. "The Diameter of Juno (3) determined with the Micrometer of the 40-inch Refractor of the Yerkes Observatory, with Remarks on some of the other Asteroids."

E. M. Antoniadi and A. C. D. Crommelin. "Observations of the Leonids made at Blackheath, 1900 November 14."

H. H. Turner. "The Leonid Observations at the University Observatory, Oxford."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1901."

T. Lewis. "The System of ζ Herculis."

W. H. M. Christie. "Observations of Capella as a Double Star with the 28-inch Equatorial of the Royal Observatory, Greenwich."

Rev. S. J. Johnson. "On the Magnitude of the Wide Companion of ζ Lyræ."

Radcliffe Observatory. "Observations of the Leonids, 1900."

The following gentlemen were elected Fellows of the Society:—

Sri Rajah Ankithum Venkata Jugga Row Bahadur, Yambram Estates, Vizagapatam, India.

Ernest W. Shackleton, R.M.S. 'Tintagel Castle.'

The following Candidates were proposed for election as Fellows of the Society:—

Rev. Geo. Gaskell-Vickars, Grange-over-Sands, North Lancashire (proposed by Washington Teasdale).

Otto Hilger, Scientific Instrument Maker, 204 Stanhope Street, Hampstead Road, N.W. (proposed by Maures Horner).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The first ordinary Meeting of the eleventh Session of the British Astronomical Association was held on Wednesday, November 28, at Sion College, *Mr. G. M. Seabroke*, President, in the Chair.

The election by the Council of eighteen new Members was confirmed, and the names of ten Candidates were read and passed for suspension.

Mr. S. A. Saunder read a paper on "Mapping the Surface of the Moon." The writer remarked that Schmidt's map, published in 1878, was still probably the best existing, but, although the result of nearly 35 years' continuous work, it was by no means free from errors. It might therefore be reasonably concluded that the task of adequately representing the lunar surface was beyond the power of any one man to accomplish. The work had been very much neglected by professional astronomers, which, perhaps, was one reason why it was in such an unsatisfactory state, but if so, it offered an opportunity to the B. A. A. to remove the reproach. The exquisite photographs which were being taken at the Paris Observatory by MM. Loewy and Puiseux formed an admirable basis for such a work. From them it would be possible to fix the positions of 5000 points, each with an error of less than 1″ of arc. The most serious of the difficulties to be faced was the enormous wealth of detail visible, whilst much of it appeared only for a few hours at a time and at intervals of many lunations. Here the Association would be at an advantage, its observers practically encircling the globe. Mr. Saunder then proceeded to suggest a scheme for the conduct of the great work. He proposed to fix the scale at 200 inches for the Moon's diameter, and to divide it into sections of 0·1 of the Moon's radius, or 10 inches square, with an extra ½ inch all round for overlap, making each sheet 11 inches square. The principal points would have to be measured, and the outlines drawn in from the Paris, Lick, and Dr. Weinek's Atlases. Only such detail as was clearly shown in the photographs should be represented, all the rest should be filled in at the telescope. He hoped that in a year's time enough progress would have been made to render the results worth discussion. Each observer should select some particular portion of the surface, which he should study on every possible occasion and endeavour to map exhaustively.

Mr. Walter Goodacre said there were one or two points contained in Mr. Saunder's paper which he would like to emphasize. All who had any knowledge of selenographic matter would be aware of the efforts that had been made in this direction by the British Association and by the Selenographical Society, but the difficulties they had had to contend with proved in the end quite unsurmountable. Fortunately there was now a better starting-point, and photographs of the Moon were available which were almost all that could be desired. The work was one in which the possessors of the smallest telescopes could join. Everything that was visible on the Paris photographs could be seen in a 2¼-inch telescope.

Mr. C. T. Whitmell described an observation of "The Green Flash" made by him on July 26 last. The disc of the Sun was of a bright orange-yellow colour. Just before disappearance the last visible tip of the disc changed its colour to bluish green, the phenomenon lasting perhaps a couple of seconds.

Mr. Whitmell then read a paper on "The Duration of Annularity in a Solar Eclipse." After referring to the conditions of a total eclipse, the writer pointed out that in the case of an annular eclipse the annulus decreased as the Moon's altitude increased, and increased as the altitude decreased. Hence, if the observer's velocity was left out of account, annularity should be longest at sunrise and sunset, and shortest at noon, or just the opposite to what happened for totality; but in retarding the Moon's transit, the observer's velocity had a maximum effect at noon, just when the annulus was narrowest, and a minimum effect at sunrise and sunset, just when the annulus was broadest. There were thus two factors conflicting, and it was impossible to say, without further consideration, whether annularity at noon would be longer or shorter than at sunrise or sunset. He showed that there was a certain distance of the Moon which made the duration of annularity constant throughout the eclipse. When the Moon's distance was greater than this, the duration of annularity was longest at noon, when less it was shortest at noon. The annular eclipse of February 1, 1897, admirably illustrated the case of a nearly constant annularity.

Mr. A. C. D. Crommelin said it had puzzled him for some time how, in the cases of some annular eclipses, the duration of the annularity was greater at noon, when the Moon looked bigger, than at sunrise and sunset. However, the discordance was now explained by Mr. Whitmell's result. There was a certain value of the distance of the Moon, on one side of which the duration of annularity was longest in the middle, and on the other side shortest in the middle.

Capt. Wm. Noble then described Prof. Burckhalter's method of photographing the whole corona by means of a couple of revolving cameras. The photographs of the corona taken by this method and presented to the Association by Prof. Burckhalter were exhibited on the screen.

Mr. Crommelin remarked that Prof. Burckhalter's were the only photographs in which he had been able to recognize the three more brilliant regions which he had seen in the lower corona.

Mr. Thorpe suggested a method for photographing the corona, which in his opinion was simpler than Prof. Burckhalter's. By placing a vignette arrangement in front of the photographic plate, the other parts would fade away gradually, and thus an even illumination of the corona would be obtained.

Mr. Maunder read a paper on "Photographs of the Partial Phase" by himself and Mrs. Maunder. One great object they had in view in the recent eclipse was to push a little further the experiment which they had made in India on photographing the corona outside totality. For this purpose forty photographs were taken during the partial phase with varying instruments and exposures. These experiments had been very strikingly supplemented by the work which Mr. Nevil Maskelyne carried on with his cinematograph in America. Mr. Maskelyne's instrument had an aperture of ⅜ inch. The instrument was run for about 5¾ minutes, commencing some 25 seconds before totality, and running for nearly four minutes after totality was ended. The last photograph showing the corona was estimated at 2^{m} 12^{s} after the return of sunlight.

Mr. W. H. Wesley then read his Report on the Corona of 1900, from photographs taken during the eclipse by Members of the Association. The general aspect of the corona, as shown in the photographs, was that of a wind-vane, being somewhat broad and spreading on the west side, and pointed on the east. The corona had thus a fish-tail form on the west, remarkably like its eastern side during the eclipse of 1898. The corona of 1900 showed a much nearer approach to the type which had been usually associated with a minimum period of sun-spots than that of 1898. In its general form it most nearly resembled that of 1889 (January 1). Its most interesting feature appeared to be the occurrence of the dark rays or streaks. Among the photographs submitted for examination were negatives taken at such distant stations as North Carolina and Algiers. He had therefore especially examined them for any evidence of change in coronal forms. A careful outline had been made from the Algiers photographs, and another, quite independently, from the American negatives; but they appeared to afford no evidence of coronal change during the passage of the shadow from North Carolina and Algiers.

The cinematograph of the eclipse was then shown on the screen.

ROYAL METEOROLOGICAL SOCIETY.

THE monthly Meeting of this Society was held on Wednesday evening, December 19, at the Institution of Civil Engineers, Westminster, *Dr. C. Theodore Williams*, President, in the Chair.

Mr. H. Mellish read a paper on "The Seasonal Rainfall of the British Isles," which he illustrated with a number of lantern-slides. He discussed the rainfall returns from 210 stations for the 25 years 1866–90, and calculated the percentage of the mean annual rainfall for each season. In winter the largest percentages of rainfall are found as a rule at the wet stations, and the smallest at the dry ones. Spring is everywhere the driest quarter, and the percentages are very uniform over the country, rather larger in the east than in the west. In summer the highest percentages are found in the dry districts, and the lowest in the wet ones. As the spring is everywhere dry, so is the autumn everywhere wet, and there is little difference in the proportion of the annual total which falls in the different districts. As regards the relation between the amount of rain which falls in the wettest and driest month at any station, it seems to be generally the case that the range is larger for wet stations than for dry ones. In wet districts rather more than twice as much rain falls on the average in the wettest month as in the driest, and in dry districts rather less than twice

Changes in the Stellar Heavens.

(*Addendum.—See* Vol. XXIII. p. 454.)

Towards the end of the eighteenth century Sir William Herschel made a series of observations on the relative brightness of the stars given in Flamsteed's Catalogue, that is, the stars visible to the naked eye in England. Some of these observations were published in four catalogues in the *Philosophical Transactions of the Royal Society* for the years 1796, 1797, and 1799, and two others were found by Prof. E. C. Pickering at Collingwood in the year 1883, and are now in the library of Harvard College Observatory, U.S.A. I have carefully gone through the published observations and compared them with modern estimates and photometric measures. As might be expected from so great an observer, Herschel's observations are wonderfully accurate, and compare very favourably with modern measures. I have found, however, a number of discordances which suggest variation of light. The following are some of the most remarkable and interesting of these discordances.

Aquila.—Herschel rated 11 Aquilæ just perceptibly brighter than 10, but the Potsdam photometric measures make 11 nearly one magnitude brighter than 10. In one observation 23 Aquilæ was rated by Herschel as equal to 24, and in another observation as nearly half a magnitude the brighter of the two. Argelander rated both stars as 6 mag. Heis called 23, 6–5 mag., and 24, 6–7 mag. The *Durchmusterung* gives 5·0 and 7·0, and the *Uranometria Argentina* 5·7 and 6·8. The photometric measures make 23 more than a magnitude brighter than 24. In August 1875 I estimated 24 as 6½ or 7 mag.; Sept. 23, 1879, about 6½ mag., or perhaps nearer 7 mag., at least one magnitude less than either 23 or 27, only slightly brighter than the star to the north of it; September 18, 1881, I found "24 about 7 mag. and equal to the star north of it—at least one magnitude less than 23 or 27"; September 19, 1884, 24 slightly brighter than the star north of it.

Cygnus.—Herschel rated 36 Cygni about 0·4 mag. brighter than 27, but the photometric measures make 27 slightly the brighter of the two. Argelander and Heis agree with the photometers.

Pegasus.—Herschel rated π Pegasi about 0·4 mag. brighter than Fl 27, but the Harvard measures make π more than a magnitude brighter than the other. π has been suspected of variation, and Argelander and Heis found 27 faint.

Aries.—Herschel rated 20 Arietis only just perceptibly brighter than 11, but the Potsdam measures make 20 (5·98 mag.) about 1½ magnitude brighter than 11 (7·47 mag.). 11 is 6 mag. in Harding's Atlas (1822), and has a 7 mag. star closely north of it.

It was rated 6 mag. by Flamsteed. Herschel rated 63 Arietis only just perceptibly brighter than 65, but the Potsdam measures make 63 over one magnitude brighter than 65. The Harvard measures make 63 a little the brighter of the two, and the Oxford measures make them very nearly equal. On October 3, 1899, I found 63 about half a magnitude brighter than 65, and about the same on October 5, 6, 8, and 12, 1899.

Cetus.—Herschel rated 10 Ceti only just perceptibly brighter than 11. The latter star is not in the *Uranometria Argentina*, nor is it given by Heis, but 10 was rated 6·2 mag. at Cordoba. On October 3, 1883, I estimated 11 as 7·3 mag., so that if Herschel's observation was correct, the star is probably variable.

Eridanus.—The star 19 Eridani was rated by Herschel just perceptibly brighter than 12, but modern estimates and measures make 12 distinctly brighter than 19. The Cordoba observations make 12 (3·6 mag.) nearly one magnitude brighter than 19 (4·5 mag.).

Boötes.—The stars β and δ Boötis have both been suspected of variation. Herschel rated δ, in one observation, slightly brighter than β, and in two other observations he found β the brighter of the two. The photometric measures make δ slightly brighter than β. Dr. Gould states (*Astronomische Nachrichten*, 1620) that "Mr. Chandler is confident that he has seen δ Boötis vary." Heis' observations (1844–1849) show δ from one to four steps brighter than β. On March 23, 1884, I found δ one step less than β; September 10, 1899, β one step brighter than δ, and three steps less than γ Boötis.

Cepheus.—Herschel rated 28 Cephei distinctly brighter than 29 (ρ), but the photometric measures make ρ distinctly brighter than 28. As the stars lie close together there should be no difficulty in estimating their relative brightness.

Auriga.—Herschel found 26 Aurigæ just perceptibly brighter than 22, but the Potsdam measures make 26 one magnitude brighter than the other. The stars are near each other and should be easily compared. 26 has been suspected of variable light, but Herschel's observations point to 22 as probably the variable.

Draco.—κ and λ Draconis were rated equal by Ptolemy, Al-Sufi, Herschel, Argelander, and Heis; and the photometric measures make them nearly equal. My own observations (1876–1900) show considerable variation in the relative brightness of the two stars. On June 12, 1877, I found λ slightly brighter than κ, but on December 23, 1899, I estimated κ to be one magnitude brighter than λ. One of the two stars seems to be variable, and the variable is probably λ, as it is of a reddish colour, and its spectrum is of the third type. With reference to the star η (14) Draconis, Herschel remarks in the notes to his observations, "M. de la Lande says the star is not to be found. See Mr. Bode's *Ast. Jahr-Buch* for 1795, p. 198. I observed this star in a sweep of the heavens

June 2nd, 1788. Its brightness was estimated September 11, 1795, September 24, 1796, September 30, 1796, and December 28, 1798; so that if M. de la Lande is sure no cloud intervened when he looked for it, we may suspect it to be a changeable star." The star was, however, seen by Ptolemy, Al-Sufi, Flamsteed, Argelander, and Heis, and was measured at Oxford and Harvard. It is now about 3rd magnitude, or slightly brighter. J. E. Gore.

The November Taurids.

I obtained my first view of these meteors in 1872 November 6–10, when I found the radiant at 64° + 20°, and they were again conspicuously visible in 1876. It became evident that there were two pretty rich showers, one at 57° + 9° and the other at 63° + 22°. They are visible from August to December. The former is very active during the first half of the month; the latter during the last half. In this note it is proposed to deal with the former shower.

These November Taurids are often very brilliant, and are occasionally seen as detonating fireballs of the largest type.

This year, while watching for Orionids, October 23, 26, and 27, I recorded about six meteors from the radiant at 57° + 9°, and in November, at the Leonid epoch, there were some beautiful meteors from a radiant at 58° + 10°. This particular shower is usually the richest display of slow-moving meteors seen near the period of the Leonid display. In ordinary years the month of November certainly furnishes more Taurids than Leonids, for I found, on consulting my records between 1876 and 1900, that there were 177 Taurids, 83 Perseids, and 118 Leonids registered in the month named.

In the following list I give some of the brighter Taurids recorded here from a radiant at about 58° + 10° in different years. The position is a diffuse one, extending over an area of some 10 degrees, but errors of observation are partly responsible for this diffusion. Some of the meteors were observed casually, and therefore recorded not very accurately. Several of the tracks which, on being carried backwards, cut through the Hyades, ought probably to run just below that group. When the eye has to roughly estimate the backward line of flight without any guide, a meteor directed from the region of a star or star-group will be marked down as having that precise direction. In the absence of a straight wand to project upon the track, it is impossible to fix the positions correctly, especially when the object is far from its radiant. The apparent diffusion of the radiant may be due to the existence of a shower at about 58° + 16°, which seems well supported by a number of observations.

Radiant near λ *Tauri* (58° +10°).

Date.	G.M.T.		Mag.	Path.				Length.	Notes.
				From		To			
	h	m		a.	δ.	a.	δ.		
				°	°	°	°	°	
1900 Oct. 23 ...	10	42	1	335	+49	296	+42	28	Medium velocity.
1879 29 ...	9	50	♀	38	+18	26	+22	12	B. flash, broken streak.
1893 31 ...	6	44	♃	4	+10	326	+ 4	38	V. slow, B. T.
1877 Nov. 1 ...	8	58	♃	44	+56	245	+51	68	Slow, yellow, T.
1889 1 ...	11	37½	1	50¾	− 6	50	+11	5	Slow.
1890 1 ...	7	1	>1	278	+49	244	+11½	46	Slow, star-like.
1 ...	9	17	>1	345	+11	307	+ 1½	39	Slow.
3 ...	9	0	>1	nearly sa		me	path	as	foregoing.
1886 2 ...	12	10	1	32	+ 5	23	+ 1	10	V. slow, yellow, T.
1877 4 ...	12	9	1	73	+15	83	+19	10	Slow, streak.
1896 5 ...	8	22	1	25	+88	222	+74	18	Slow.
1879 6 ...	6	43	>1	21	+16	349	+ 9½	22	Slow, T.
1877 7 ...	16	32	1	75	+27	91	+35	16	Slow.
1876 8 ...	6	29	>1	339	+29	289	+ 9	50	Rather slow, T.
8 ...	6	51	1½	329	+30	284	+10	46	Rather slow, T.
8 ...	8	53	1	8	+19	346	+17	20	Streak, 5 sec. acc.
1874 10 ...	8	38	1	134	+48	153	+46	13	Slow.
1879 12 ...	11	18	1	38½	+27	27½	+32	11	V. slow, T.
1900 13 ...	10	17	1	58	+19	58	+22	3	V. v. slow.
13 ...	11	40	>♀	11	+51½	318	+54	31	Rather slow, yellow, T.
1879 14 ...	11	2	1	189	+82	217	+68	15	Slowish.
1900 14 ...	12	45	>♃	31	+62	290	+64	41	Slow, yellow, T.
1885 15 ...	14	18	♃	197	+62	212	+47	17	Slow.
1880 25 ...	6	42	1	319	+58	289	+51½	19	Slowish, B. T.

B., bright; T., train; V., very.

The shower is a most important one, and its meteors bright and attractive. They move slowly and often have very long flights, during which the nuclei cast off trains of yellow sparks. In the early part of the evening, when the radiant is low, the paths sometimes extend over 40° or 50° or more. For several reasons the shower is one meriting the close observation of future observers, for, apart from the Leonids and Andromedids, it is probably the strongest display presented to us in the evenings of November, while it often furnishes brilliant meteors and occasionally fireballs of the largest class. Its radiant-point, like that of the Andromedids, really seems very diffuse and needs redetermination. The long duration of the shower, through most of the summer and autumn, is another interesting feature in connection with it.

My various determinations of the radiant are as under:—

	α.	δ.	
	°	°	
1900 July 23	54	+ 9*	S. S. P.
1877 Sept. 4–16......	59	+ 9	5 meteors.
1879 „ 14–21	54	+10	6 „
1898 „ 17........	57	+ 7*	S. S. P.
1900 Oct. 23–27	57	+ 9	6 meteors.
1886 Nov. 2–3	55	+ 9	17 „
1899 „ 10........	55	+ 5*	S. S. P.
1900 „ 13........	58	+ 7	S. S. P.
1900 „ 13–14	58	+10	6 meteors.

* Radiant derived from a doubly-observed shooting-star.

Bishopston, Bristol,
1900, November.

W. F. Denning.

The Origin of Terrestrial Magnetism *.

Dr. Schmidt has just completed an important harmonic analysis of the permanent magnetic field of the Earth. His researches are, in a sense, an amplification of the great work of Gauss, *Théorie générale du magnétisme terrestre*. Whether Gauss had propounded any *theory* whatever has been considered doubtful by some, so that the question "What was the opinion of Gauss upon this subject"? may be judged to be not without interest. As for me, I should be tempted to reply, as Herz did in his book upon electric waves, when some one asked him "What is Maxwell's electro-magnetic theory of light"? He replied, "Maxwell's theory is contained in the equations by which he defines the magnetic field." I would say in the same way, "Gauss's theory is contained in his equations, consisting of spherical harmonic terms to define the permanent magnetic field of the Earth." But what results from these equations? They comprise the following theoretic statements: the permanent magnetic field of the Earth, in its entirety, results from causes residing in the terrestrial crust, causes of a character which may be expressed by a potential; of such a kind that knowing the value of this potential, all the components are known, one in the north and south direction, another in an eastward direction along a parallel of latitude, and the third in a vertical direction can be easily calculated. Also if these theoretical equations when applied to the actual observations of terrestrial magnetic force are verified with accuracy within the limits of error of observation, then the assumptions underlying the equations are proved. It is this that Gauss has done, and in that, as

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* [Translation of an article in *Ciel et Terre* for 1900, Dec. 16, by Prof. Bauer, of the U.S. Geodetic Service. It seems appropriate to publish this in connection with Father Sidgreaves's paper read at the R. A. S. Meeting reported on p. 39.—Eds.]

I say, the theory consists. The equations thus represent a definite physical fact.

Dr. Schmidt, remarking that observations have considerably multiplied since the time of Gauss, has decided to submit the hypothesis of Gauss to a new trial. To this end his analysis does not assume the existence of an interior potential function governing the entire magnetic force; but he adjusts separately each of the three rectangular components, and obtains thus three expressions in place of one that Gauss has given. He carries these expressions as far as terms of the sixth order, whilst Gauss stopped at the fourth. The agreement or disagreement between the coefficients of the terms in the three separate expressions ought to verify the hypothesis of Gauss, and Schmidt arrives at the following conclusions:—

The magnetic force of the Earth consists of three parts:

1. *The greatest part* must be attributed to causes situated in the terrestrial crust: this has a potential.
2. *The smallest part, about one-fortieth of the entire force*, is due to causes exterior to the crust, and also possesses a potential.
3. *A part, a little greater than the preceding*, cannot be represented by a potential, and consequently indicates the existence of vertical terrestrial electric currents. The currents at the surface of the Earth amount in the mean to a sixth of an ampère per square millimetre.

I remark in passing that Dr. Schmidt appears a little incredulous as to the existence of the third part, and would be ready to admit that this conclusion is the result of errors of observation. I wish only to state that on the whole Gauss's theory has received complete verification.

Dr. Schmidt, indefatigable in his researches, has recently published another work upon "The Cause of Magnetic Storms."

When we compare the photographic curves obtained at different magnetic observatories, curves which show the variation of magnetic declination or that of horizontal force during a magnetic storm, the eye is struck by the manifest accordance at similar points. Careful examination of these curves shows that the accordance consists principally in the number and position of waves or summits which compose the curves, whilst the heights of the summits and depths of corresponding depressions show great differences: at one time the summit of a curve appears flattened by comparison with the other, so that the maximum of one corresponds to the minimum of the other. The more the stations are separated from one another the more frequent and more marked will be the differences between the curves; at stations very far apart the curves have very few points of resemblance. One peculiar characteristic of these fluctuations of terrestrial magnetism is the continual change which pervades them. A perfect resemblance is followed in a few minutes by a complete dissimilarity, a marked rise in one curve corresponds to an inclination scarcely sensible in another. This well-known peculiarity of magnetic storms makes it evident, without doubt, that local influences are the principal cause of the phenomena; rapid changes in the force and in the extent make their existence evident, first here, then there, and whilst the effect may be produced at one time over a large region the maximum passes rapidly from one point to another.

Dr. Schmidt has made a mathematical analysis of several magnetic storms, and in particular of that of Feb. 28, 1896, the progress of which was followed for an hour, from 6 to 7 o'clock, Greenwich time, at fifteen observatories. His researches clearly show that the directions of disturbance converge sometimes to a point, at other times radiate from a point, and during moments of magnetic calm (relatively speaking) the lines of force at different stations were nearly parallel one to another, as if they all indicated a centre of force far away. At one time the points of convergence move, generally with a velocity of about a kilometre a second, and then at another time they are nearly stationary. Taking account of the fact that the course of diurnal variation of terrestrial magnetism ought to be, judging from the circumstances, attributed to electric currents in the upper regions of the atmosphere, Dr. Schmidt thinks that the immediate cause of magnetic storms ought to be found in electric whirls, which separate in the atmosphere of the general magnetic field, so that they make cyclones and anti-cyclones well known to meteorologists. Taking also into consideration the vertical component disturbances, and applying the law of Ampère to the system of currents revealed by the disturbing causes, it follows that, for the most part, the causes which produce our magnetic storms are exterior to the surface of the Earth.

L. A. BAUER.

Researches on the Diameter of Venus *.

DURING the past two hundred years the diameter of Venus has been determined by more than fifty separate investigators, and in the course of the present century has been carefully studied by the Government parties in the transits of 1874 and 1882, as well as by numerous individual observers equipped with heliometers, filar and double-image micrometers, and other apparatus of special design; yet, in spite of all the labour which has been bestowed upon the subject, it appears that there is still no standard value in general use among astronomers.

The measurement of the diameter of Venus presents among others the following peculiar difficulties:—

1. The enormous change in the geocentric distance of the planet renders the apparent diameter extremely variable. And unfor-

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* From *Astronomische Nachrichten*, 3676.

tunately the phase is so arranged as to give merely a thin crescent when the body is nearest the Earth, and when the disc is more rounded out and diminished in angular diameter, the distance is so much increased that errors entering into the measures affect the reduced diameter greatly.

2. The line-like horns of the very thin crescent which the enlarged disk presents when near inferior conjunction are so delicate that small atmospheric irregularities set them in violent motion, and it is difficult to locate their quiescent positions with the micrometer wire. Even greater difficulty is experienced in forming accurate contacts of the images produced by the heliometer and double-image micrometer.

3. When the crescent is enlarged the horns are broadened and more steady, but the apparent diameter is less, and an error in the setting enters with enlarged effect into the final result.

4. When the planet approaches superior conjunction, and the disc is nearly round, these difficulties diminish, but the diameter is then so small that the advantage sought is more than lost, through the enlargement of errors of observation, in the final result. Practically these difficulties are augmented by the circumstance that the planet is then near the Sun, and must necessarily be observed at a time of day when the air is much disturbed by the heat.

5. During the transits across the Sun's disc, the image is both large and round, but its diameter is vitiated by unknown causes, like irradiation, a halo due to the planet's atmosphere, the black drop, &c., which, however, have been carefully investigated.

6. Venus is always extremely bright, and its light very white, which renders the irradiation large and the secondary spectrum troublesome.

The following table gives a brief summary of the principal investigations relative to the diameter of Venus :—

Authority.	Source.	Epoch.	Diam.	Remarks.
Galilei	Houzeau, V. M. 462.	1620±	″ 17·2	From observations near superior conjunction.
Horrocks	B. A. J. 1807, 165–6.	1640	17·609	Calculated by Wurm; 2 days, by micrometer.
La Hire	„ „ „	1700	17·610	By micrometer, 1 day, near inf. conj.
Short	„ „ „	1761	17·065	By micrometer, 4 days, near inf. conj.
Lalande	Houzeau, V. M. 463.	1762	16·7	Measured during transit of 1761.
Schroeter	B. A. J. 1807, 166–7.	1789–94	16·835	7 days, micrometer.
Herschel	„ „ „	1791	18·790	1 day, micrometer.
Schroeter	Phil. Trans. 1792, 317.	1792	16·7	Micrometer measures on 4 days.

Authority.	Source.	Epoch.	Diam.	Remarks.
Wurm	B. A. J. 1807, 167.	1804	″ 16·954	22 sets of micr. measures during transit of 1761.
Wurm	„ „ „	1804	16·810	8 sets of micr. measures during transit of 1769.
Arago	Œuvres, xi. 1859.	1815	16·9	Double-image micrometer.
Encke	Entf. d. Sonne, 129.	1822	16·611	From the transit of 1761.
Ferrer	Mem. R. A. S. v. 281.	1833	16·676	By duration of transit of 1769, at 15 stations.
Beer & Mädler...	A. N. 325.	1837	17·134	Obs. on 66 days; corrected for various errors.
Challis	Houzeau, V. M. 463.	1839	17·51	2 sets of measures, double-image micrometer.
Challis	„ „ „	1841	17·57	Double-image micrometer.
Airy	Mem. Obs. Paris, 1861.	1845	16·566	Micrometer measures, $D=(8''\cdot283:\Delta)+0''\cdot60$.
Lassell	Leiden. Ann. iii. 222.	1849	17·15	By heliometer, 2 days.
Hartnup	„ „ „	1849	17·92	Filar micrometer, 4 days.
Hartnup	„ „ „	1849	17·13	Double-image micrometer, 4 days.
Thompson	„ „ „	1849	17·77	1 day, filar micrometer.
Thompson	„ „ „	1849	17·58	1 day, double-image micrometer.
Wichmann	A. N. 749.	1851	17·325	By Königsb. heliometer, 11 days.
Peirce	A. J. 50.	1852	17·092	Wash. mural circle obs. 1845–6, 30 days.
Main	Mem. R. A. S. xxv. 46.	1855	17·55	20 series of microm. measures, 1840–52.
Gilliss	U.S.N. Exp. to Chili, iii.	1856	16·70	Micrometer measures, discussed by Gould.
Secchi	A. N. 1089.	1857	16·42	1 day, at inferior conj.
Schmidt	A. N. 1543.	1865	17·18	39 days, micr. meas. corr. for irradiation, $0''\cdot15$.
E. J. Stone	M. N. xxv. 59.	1865	16·944	Mural Circle and Merid. obs. at Greenwich, 1839–62. 589 obs.
Powalky	A. N. 1841.	1871	16·918	Transits of 1761 and 1769.
Vogel	Bothk. Beob. ii. 1873.	1871	16·867	8 days, by micrometer.
Kaiser	Leiden Ann. iii. 221.	1872	17·516	34 days, in 1862 and 1865. Double-image micr. of Airy.
Plummer	M. N. xxxiii. 561.	1873	17·321	26 days, Durham Observatory, irradiation $0''\cdot273$.
Auwers	Venus Durchg. 1874.	1874	16·957	Discussion of observations at Luxor, &c.
Tennant	M. N. xxxv. 347.	1875	16·903	Double-image micr. mea- during transit, 1874. 6-inch telescope.
Downing	M. N. xxxvii. 399.	1877	16·738	From Wash. transit-circle obs. 1866–72. Well discussed.
Hartwig	Publ. d. AG. xv. 10.	1879	17·666	By Breslau heliometer.
Thackeray.........	M. N. xlvi. 336.	1886	17·010	From Greenwich obs. 1873–84.

Authority.	Source.	Epoch.	Diam.	Remarks.
Auwers	A. N. 3068.	1891	16″·801	Mean of results for 1874 and 1882, by heliometer; diameter of solid planet.
Auwers	A. N. 3214.	1894	16·820	A correction in above leads to this final value.
Ambronn	A. N. 3204.	1893	17·711 ±0·047	34 obs. with Gött. heliometer.
Barnard	Pop. Astron. 1897–8.	1895	17·397	24 days near superior conj., when obs. D varied from 14″ to 25″.
See	A. N. 3676.	1900	16·800	32 days, 26-inch Washington refractor.

The value 16″·820 for the diameter of Venus, deduced by Dr. Auwers in 1894 from the transits of 1874 and 1882, is especially worthy of attention. Besides resting upon a most rigorous and exhaustive discussion of all available material, it has the advantage of resulting from observations taken when Venus had a diameter of 63″·5. Under these circumstances, errors in the observed diameters affect the value at the mean distance by only about one fourth of their original amount. Though the elimination of such influences as the irradiation is difficult and attended with some uncertainty, it hardly seems possible that his value of the diameter can depart from the truth to any great extent.

In examining the Table one cannot fail to be impressed with the large values given by several recent determinations with the heliometer. This is the more singular, since in the case of other planets like Jupiter and Saturn the diameters found with this same instrument have usually come out abnormally small.

Is it possible that the crescent-like figure of Venus in an instrument whose images depend on half lenses is the source of this anomaly? Or is it due to the unsteadiness of the horns blurred by atmospheric irregularities into a violent quiver, which prevents strict tangency, and thus makes the measured diameter too large by an amount depending on the blurring, which is itself a function of the thickness of the crescent as well as of the seeing?

In adjusting their observations, some previous investigators have formed equations of condition, arranged in such a way as to produce a term depending on an irradiation which varies with the phase of the planet. Having followed Venus attentively from superior to inferior conjunction, the writer finds it difficult to believe that this procedure is well founded. The brightness at the horns appears always sensibly the same from east to west elongation. And the causes which produce the chief errors are evidently those connected with the appearance of the horns of the crescent when disturbed by the irregularities of the terrestrial atmosphere. T. J. J. See.

[We have a letter from Mr. Lynn on the diameter of Venus in which he says:—"Commencing with 1896, the mean value 16″·80 has been used in the *Nautical Almanac*, which was deduced by Prof. Auwers in 1891. It appears to have been overlooked that in 1894 Prof. Auwers published a correction of this value, giving as his final result 16″·82."—Eds.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1901.

Gentlemen,—

The first predicted appearance of Encke's Comet took place in 1822, at which return it was only seen in New South Wales. And it is interesting to remember that that was the second case of the observed return of a comet according to prediction, 63½ years after the re-discovery of Halley's Comet at Christmas in the year 1758. The third case was the return of Biela's Comet in 1832. That body was not seen at the return in 1839, when it was unfavourably placed for observation; at its return in 1845–6 its famous duplication had taken place, and both portions returned in 1852, but since that date the comet has not been seen at all, at any rate as a comet, and this is not the place to dwell upon its connexion with the Andromedes or Bielid meteors.

Encke's Comet has been observed at every return since 1818, when that great astronomer made a complete determination of its path, which enabled him to show, on the occasions of several subsequent appearances, that its period of revolution was regularly diminishing by about 2½ hours at each return to perihelion. This effect he attributed to the retarding action of a resisting medium in space. This explanation for many years appeared to be satisfactory, though no such effect could be substantiated in the motion of any other comet. But some time after Encke's death the diminution in the length of the period was reduced by one half, which is fatal to his theory with regard to its cause. Additional interest is thus imparted to future discussion of the motions of this remarkable body, which is now regarded as an old and constant friend, and occasionally deigns to become just visible without the aid of a telescope. The last return to perihelion took place on Her Majesty's birthday in 1898—the same day on which it also occurred at the first predicted return in 1822, when it was sighted by Rümker at Paramatta nine days afterwards, on June 2. It follows that another perihelion passage will be due in the middle of September 1901.

Another comet which calls for mention here seems to have passed through a somewhat chequered history. Discovered by

Brorsen in 1846, it was afterwards observed at several returns, not all consecutive, the last of which was early in the year 1879. Its subsequent non-appearance is probably due to some catastrophe not yet fully explained. The period being about 5½ years, four of these have nearly elapsed since the last appearance, and Herr Berberich has published an ephemeris in *Ast. Nach.* No. 3670, according to which it should return to perihelion about the 18th of January, and make its nearest approach to the Earth a few days before that (the apparent place being in the constellation Capricornus); it is impossible, however, to feel hopeful that it will be seen. Some remarkable connexion has been suggested between this comet and one which was discovered by Mr. Denning at Bristol on the 26th of March, 1894, and a good deal of discussion took place on the subject in the following year. That comet was calculated to have a period of about 7½ years, which would bring it into perihelion again in the late summer or early autumn of 1901. But Mr. Denning himself pointed out in a letter to the *Observatory*, vol. xviii. p. 199 (May 1895), that, even if it does return at that time, it is exceedingly unlikely to become visible on account of its position. It appears, then, that Encke's is the only periodical comet we can really look forward to seeing next year.

Yours faithfully,

Blackheath, 1900, Dec. 12. W. T. LYNN.

W. Herschel and A. Wilson.

GENTLEMEN,—

In my article in your December number, "One Hundred Years ago," I remarked that though Herschel would probably have accepted Wilson's theory of the solar spots, I could not find that he had actually alluded to it. Prof. Holden, in his biography 'Sir William Herschel,' states (p. 146) that he does so in his paper on the "Nature and Construction of the Sun and Fixed Stars," published in the *Philosophical Transactions* for 1795. I can find no mention of Wilson's name in that paper (Prof. Holden's quotations seem rather free). But I do find that I had overlooked a reference to him in the paper from which I quoted, in *Phil. Trans.* for 1801. It is in a footnote on p. 270, which runs thus:—"For a geometrical proof of the depression of the nucleus of a *spot*, as an opening was formerly called [Herschel does not seem to doubt that his proposed alteration of the designation would be accepted], see a most valuable paper of Observations on the Solar Spots, by the late Alexander Wilson, M.D., Professor of Practical Astronomy in the University of Glasgow, *Phil. Trans.* vol. lxiv. part 1." Wilson had died in 1786, having resigned his professorship two years before, at the age of seventy.

Yours faithfully,

Blackheath, 1900, Dec. 11. W. T. LYNN.

Baily's Beads and the Moon's Limb.

GENTLEMEN,—

On p. 431 of the current number of the *Observatory* I find myself reported as having said that "It seems to me that the edge of the Moon cannot be as the edge of a circle," which reads curiously like nonsense. What I really did say, *apropos* of the formation of Baily's beads, was that the limb of the Moon could not be like the edge of a circular *saw*—meaning, of course, that it could not be serrated so perfectly symmetrically as to exhibit a series of beads of light of the same size and shape when the Moon was just superposed on the Sun.

While on the subject of errata, perhaps I may point out that on p. 29 of the *Companion to the Observatory for* 1901, the phase interval of Algol is given as 2 hours, 20 minutes, 49 seconds, when, very obviously indeed, 2 days, 20 hours, and 49 minutes is intended. I am,

Faithfully yours,

Forest Lodge, Maresfield,
Uckfield, 1900, Dec. 18.

WILLIAM NOBLE.

[We offer our thanks to Capt. Noble for his corrections, and express regret that the report should have been in such contradiction to his meaning.—EDS.]

"Green and Welsh," 1852.

GENTLEMEN,—

I have been asked if I can throw any light upon the circumstances attending Green and Welsh's balloon ascent in 1852, alluded to by the Rev. Mr. Bacon.

Welsh's statement (*Phil. Trans.* 1853, pp. 319–320) is very precise. In the account of his fourth ascent on 1852 November 10 Mr. Welsh says:—"Bearings and altitudes taken by Mr. Glaisher show that at $2^h\ 44^m$, when the height was 11,000 feet, the balloon was 5 miles S. by E. of Greenwich Observatory." He then gives details with regard to the greatest elevation attained by the balloon, its place of descent at Acryse, near Folkestone, and the circumstances attending the descent. But nothing in this part of the account relates to Mr. Glaisher or to the remarkable power of his telescope as stated by Mr. Bacon. I believe the telescope used by Mr. Glaisher on this occasion was the Sheepshanks Equatorial. Yours faithfully,

Blackheath, 1900, Dec. 26. W. C. NASH.

PUBLICATIONS.

BY LAND AND SKY *.—An account by the Rev. John M. Bacon of his balloon voyages. These aerial journeys were undertaken to carry out experiments on sound under varying conditions, on wireless telegraphy, and to observe the Leonids. It is the latter which ought to appeal most strongly to us, but as the Leonids failed to keep their appointment, and as the whole book is so very readable, somehow the astronomical craving gets very much in the background. The enthusiasm of the author is at once evident, fatal accidents to former aeronauts are described in such a way that one begins to look upon them as luxuries. He also seems to have a keen sense of the humorous side of all the scrapes he gets into, and so contrives to make situations which may at any instant be fatal slip by as mere casual events. Of course there is a brief history of ballooning, which is naturally traced back to Roger Bacon. An account of Green's voyage across the Channel to the German forests—by the way, the date of this venture is not given. A note on Green's voyage in 1852 is of peculiar interest to us:—"From the roof of Greenwich Observatory, by means of a good telescope, a spectator had seen the balloon rise from Vauxhall, climb the sky to nearly 23,000 feet, and settle earthwards far down in the E.S.E. near Folkestone. The onlooker was Mr. James Glaisher." We must make enquiries about that telescope. The description of his own voyages in balloons, in cathedrals, in churches, in Trinity boats to lighthouses, is most vivid, and brimful of the most varied information, and yet withal light reading. Perhaps the most critical time was the ascent in search of the Leonids, when the balloon was aloft for ten hours and reached an altitude of two miles. Although the voyagers failed to see the falling stars they undoubtedly saw stars when they arrived at Earth, which happened in so rough a manner that the lady aeronaut had her arm broken. A book really worth reading.

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* 'By Land and Sky.' Isbister & Co., 1900. Price 7*s.* 6*d.*

NOTES.

COMET NOTES.—A new comet (*c* 1900) was discovered by M. Giacobini at Nice on December 20. R.A. $22^h\ 32^m$, S. Dec. $22°\ 0'$, daily motion $+6^m$, S. $8'$. Its position on Dec. $24^d\ 8^h\ 43^m{\cdot}4$ G.M.T. was $22^h\ 57^m\ 43^s{\cdot}3$, S. $22°\ 42'\ 42''$. A. C. D. C.

MINOR PLANET NOTES.—Two new planets, FT, FU, were discovered by Wolf at Heidelberg on December 20. Both are of the 13th magnitude.

Continuation of the ephemeris of Eros, for Berlin midnight.

	R.A.			N. Dec.	
	h	m	s	°	'
Jan. 3....	2	9	3	37	4
5....	2	14	27	36	12
7....	2	20	5	35	21
9....	2	25	55	34	29
11....	2	31	57	33	38
13....	2	38	10	32	47
15....	2	44	33	31	56
Jan. 17....	2	51	6	31	5
19....	2	57	47	30	14
21....	3	4	36	29	24
23....	3	11	32	28	34
25....	3	18	34	27	44
27....	3	25	43	26	55
29....	3	32	55	26	5

In spite of cloudy weather a considerable amount of material for a parallax-determination has already been accumulated, and more will probably be obtained in January, the planet being then at its brightest (8½ magnitude). A. C. D. C.

DOUBLE STARS.—Mr. Cogshall is doing good work with the 24-inch Lowell refractor. In the *Astronomical Journal*, No. 486, he publishes his re-observations of Dr. See's double stars between R.A. 18ʰ and 24ʰ. Possible changes may have occurred in λ 348, 370, 388, 435, 469, and 474, and might repay looking at by Southern observers. In general these are stars with faint companions, as will be seen from the rough analysis below:—

Separation	Under 0″·5	0″·6–1″·0	1″·1–2″·0	2″–5″	Over 5″
Comp. fainter than 10 mag.	0	1	8	23	54
Brighter than 10 mag.	5	7	10	[illegible]	5

THE OPPOSITION OF EROS.—It is needless to repeat that the procedure for determination of the parallax of this small planet now in opposition consists in measuring the difference of its position on two photographic plates, which difference, if the plates were taken simultaneously at different places, is due wholly to parallax, or, if they were taken at different times, to parallax combined with motion of the planet in its orbit. This could also be effected by measures with a micrometer on a visual telescope, but in any case the actual method will be to measure the position of the planet from surrounding stars, whose places will have to be accurately determined with reference *to one another*, so that they form the fiducial points of a network amid which the planet threads its way, and by means of which all its motions, real or apparent, can be studied. These stars are called "comparison stars," and are in general faint, as the number of bright stars that the planet approaches is limited. There is also another series of selected stars, called "étoiles de repère" (reference stars), of magnitude about 9·0 and brighter, of which several will appear on each plate, and these will be used for determination of the plate constants, so that the absolute place of the comparison stars and of the planet will be determined by means of their places. It is therefore necessary that the R.A. and Declination of these reference stars should be known with accuracy.

The most recent circular (No. 6) issued by the Committee which has the observation of Eros under its care relates almost entirely to these comparison stars and *étoiles de repère.* It states:—

(1) The observatories of Charkow and of d'Abbadia, under the direction respectively of L. Struve and l'Abbé Verschaffel, have been added to the list of institutions which are observing the reference stars, and that M. Rodrigues, Director of the Lisbon

Observatory, reports that his share of this work is well advanced.

(2) It is suggested that the work may be limited by confining the comparison stars to a zone 20′ wide, which envelops symmetrically the apparent path of Eros in the sky.

(3) It is necessary to consider all the stars in such a zone, even as faint as magnitude 12·5, as comparison stars, for certain astronomers, Profs. Barnard, Brown, and Howe, making micrometer measures, have been obliged to use comparison stars as faint as this.

This last leads to consideration of the question as to what exposure is necessary for the special series of plates which is being made at some observatories for determination of the positions of the comparison stars. From examination of plates of the Astrographic Catalogue and plates of the Pleiades taken by M. Trépied, it is thought that five or six minutes exposure is sufficient to give stars of the required faintness, and it is suggested that once every evening one plate should be taken of the region containing Eros, with exposure of six minutes, which would, in the first place, help to determine the places of the comparison stars, and would also furnish material for investigating questions relating to the trail—whether, for instance, the point measured as its centre is actually the point corresponding to the central instant of the exposure.

Besides the matter which is briefly summarized above, and a record of some work done, which will be continued from time to time, the circular contains an ephemeris of 4 circumpolar stars and 77 other stars lying along the path of Eros, from Newcomb's Catalogue, which may be used as fundamental for reduction of meridian observations.

THE ZERO OF A POSITION-MICROMETER.—In the circular above mentioned there is a plan suggested by Prof. Comstock for determining accurately the reading for zero of a position-circle. Supposing P_0 to be the approximate reading when a star runs along the fixed wire of the micrometer; separate the movable wire from the fixed by a small interval, and having set the position-circle at P_1, about 3° greater than P_0, note the time of passage, τ_1, of a star across the interval between the wires. Then set the circle at a reading P_2, about the same amount *less than* P_0, and again observe the time of passage, τ_2, of the same star. The accurate setting for zero of the fixed wire will be given, excluding refraction, by the formula

$$P_0 = \tfrac{1}{2}(P_1+P_2)+\tfrac{1}{2}(P_1-P_2)\frac{\tau_1-\tau_2}{\tau_1+\tau_2}.$$

As check of the sign of the second term, it will easily be seen that if τ_1 is greater than τ_2, P_0 is nearer to P_1 than to P_2.

The Astronomical and Astrophysical Society of America.—The second meeting of this Society took place in June last, and the account of the proceedings, which has recently reached this country, contains much that is of interest. The Meeting received the report of the committee on the observations of the solar eclipse, and the discussion which followed is reported verbatim. Prof. Newcomb opened a discussion on the observations of Eros at opposition by remarking that astronomers are divided into two classes—those who talk about things to be done and those who go to work and do them; and he feared that so far as observing Eros was concerned American astronomers would be in the first class, because, as he explained, the only observatory in America very favourably placed is Arequipa, and this has no heliometer; but no doubt Prof. Newcomb was taking an extreme view of the case. Mr. S. J. Brown spoke strongly in favour of making micrometric observations, but he explained that the parallax could not be determined from micrometer observations by the diurnal method, so that these could only be used in conjunction with nearly simultaneous observations in Europe; it seems therefore likely that his scheme will not be realized. Prof. Campbell presented papers on spectroscopic subjects, and among other papers two were sent from the Yale Observatory, from one of which the following note is abstracted.

The Parallax of Faint Stars.—It may be remembered that the chief work of the Yale Observatory has been, for some time, the determination of stellar parallax with the heliometer. The results for the brightest stars in the northern hemisphere will be found in our number for 1898 January, but after these were completed it seemed good to Dr. Elkin and Dr. Chase to determine the parallax of faint stars which had large proper motion, and a list of nearly a hundred stars, whose parallax had not been determined but whose annual P.M. was as great as 0″·5, were selected from Porter's Catalogue. It was hoped that among so large a number some very near neighbours would be found; but if there were not, it would afford some satisfaction to know that there are probably no more stars in the northern hemisphere within a certain distance. A preliminary classification of the results is as follows:—

Parallax.	No. of stars.
0″·20 to 0″·25	2
0 ·15 „ 0 ·20	6
0 ·10 „ 0 ·15	11
0 ·05 „ 0 ·10	24
0″·00 to 0″·05	34
−0 ·05 „ 0 ·00	8
−0 ·10 „ −0 ·05	5
−0 ·15 „ −0 ·16	2

Optical Tables and Data.—We have received from Messrs. Spon, 125 Strand, a most useful book with the above title, compiled by Prof. Silvanus Thompson. It is said to be for the use of opticians, but though this may be the first purpose of the book it will probably prove of equal value to the astronomer and the physicist. The book opens with tables of logarithms and numerical functions, followed by others of wave-lengths and frequencies, refractive indices and similar optical data. After this the book becomes a treatise on optics, giving lens-formulæ and much information of a numerical and practical kind, with several pages which will be of service to photographers. The price of the book is six shillings.

UNIVERSAL TIME.—It is announced that Eastern European Time, *i. e.* 2 hours fast on Greenwich, has been adopted in Egypt. Since October 1 last, the time-balls at Alexandria and Port Said, which had been dropped according to local time, have been dropped according to Eastern European Time by electric signal from the new Abbassia Observatory.

NEW VARIABLE STARS.—The following discoveries of Variables have been announced in recent numbers of the *Ast. Nach.*:—

Star in Lyra (Stanley Williams).—R.A. $18^h\ 54^m\ 22^s$, Dec. $+34°\ 45'$ (1855·0). 1899 Sept. 28, mag. 11·0; 1900 Sept. 3, mag. 9·3; 1900 Oct. 21, mag. 10·4.

Star in Aquila (Anderson), B.D. $+9°·4205$.—R.A. $19^h\ 33^m\ 48^s·2$, Dec. $+9°\ 35'$ (1855·0). 1900 Sept. 18, mag. 9·2; Oct. 1, mag. 9·4; Oct. 25, mag. 10·0; Nov. 9, mag. 10·6.

Star in Pegasus (Anderson).—R.A. $21^h\ 6^m\ 15^s$, Dec. $+12°\ 21'$ (1855·0). 1900 Sept. 26, mag. 9·1; Oct. 27, mag. 9·5; Nov. 10, mag. 10·1.

SUN-SPOTS AND RAINFALL.—Sir Norman Lockyer has returned to the fascinating subject of the connection between sun-spots and terrestrial weather. The following* appears to be the gist of a paper presented to the Royal Society on November 22:—

> The coincidence, during the last few years, of an abnormal state of the Sun with abnormal rain in India, accompanied by the worst famine experienced during the century, had suggested the desirability of reconsidering the question, especially as they had now some new factors at their disposal. These had been revealed by the study, now extending over twenty years, of the widened lines in sun-spots as seen in the spectroscope. It was shown that these widened lines were at some periods easily traceable to known elements, whilst at others their origins had not been discovered, so the latter had been classed as unknown lines. If they compared these periods with the sun-spot curve, as constructed from the measurements of the mean spotted area for each year, it was found that when the spotted area was greatest the widened lines belonged to the "unknown" class; while when the spotted area was least they belonged to the "known" class. Experiments showed that the former group of lines indicated a period of sun-spot maximum to be one of high solar temperature, whilst the reverse of this was indicated by the "known" class. In other words, an

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* From the *English Mechanic.*

examination of the chemical origin of the lines most widened in sun-spots at maxima and minima periods showed that there is a considerable rise above the mean temperature of the Sun around the years of sun-spot maximum, and a considerable fall around the years of sun-spot minimum. It had been found from the actual facts of rainfall in India (during the south-west monsoon) and Mauritius, between the years 1877 and 1886, that the effects of these solar changes were felt in India at sun-spot maximum, and in Mauritius at sun-spot minimum. Of these, the greater was that produced in the Mauritius at sun-spot minimum. The pulse at Mauritius at sun-spot minimum was also felt in India, and gave rise generally to a secondary maximum in India. India, therefore, had two pulses of rainfall, one near the maximum and the other near the minimum of the sun-spot period. The dates of the beginning of these two pulses on the Indian and Mauritius rainfall were related to sudden remarkable changes in the behaviour of the widened lines.

A writer in the *Times* says in comment on this:—

> Although a good deal of work has been done on the subject, it cannot be said that any very definite result has been reached, or that there is any accepted belief among men of science as to the validity of the hypothesis. . . If we suppose the excessive temperature of the spots to be established, the fact remains that their area is insignificant, and this is met by saying that it is the great area of accompanying protuberances which really accounts for the increase in the Sun's heat. But it has not been proved in any way that these protuberances share in the excessive temperature of the sun-spots. Yet the proof ought to be easy, because the spectroscope can be applied to them as easily as to the spots. It seems evident that from discussion of the meaning of lines in the spot spectrum we cannot infer anything about the temperature of an enormously greater area of the Sun's surface. What is really proved if we assume that the spot spectrum shows known substances in a superheated condition is that when spots are numerous they are also hot, and that when not numerous they are cooler. Since they may be either above the mean or below it, they offer no means of estimating the temperature of vast circumjacent areas. These areas are there to speak for themselves, and it ought not to be assumed that they are excessively hot even if the spots are proved to be so.

EARTH-MAGNETISM.—As want of space compelled the omission of the small table of Magnetic Elements in the *Companion* for the current year, it may be advisable to put the figures on record here:—

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, „	16 29 „	0·1844	67 8
1901, inferred	16 23 „	0·1846	67 7

The horizontal force is given in C. G. S. measure.

THE VELOCITY OF LIGHT.—A new determination of the velocity of light is in progress at the Nice Observatory by Fizeau's method with the toothed wheel. The actual apparatus used by M. Fizeau in his original experiments has been set up in the Nice Observatory on Mont Gros, with the reflecting collimator in the village of Gaude, 11,862·17 metres away. Preliminary results are given in a note by M. Perrotin in the *Comptes Rendus*, which give 299,900 kilometres per second as the velocity, with a probable error of ± 80 kilometres. It may be remembered that the most recent determination, namely that by Newcomb in the years 1880–1882, gave 299,860 kilometres per second, with a Probable Error of 30.

PROF. WOLFER's discussion of sun-spots and variation of declination needle from 1889 to 1899 inclusive brings out a remarkable agreement between the two curves.

PROF. TORVALD KÖHL, Odder, Denmark, writes that he has received, from 50 stations in Denmark and Germany, observations of a large meteor, Dec. $16^d\ 3^h\ 40^m$ P.M., Greenwich Time. He would be glad to receive any observations that may have been made in England, as it was seen in the west.

WE understand that in binding up the December number some copies are spoiled by the insertion of a number of pages of the *Companion* instead of the proper pages. We should be glad to hear from those of our readers who received such copies, in order that proper ones may be supplied.

DR. SEE, in continuation of his researches on the figure and dimensions of the planets, gives in *A. N.* 3670 a complete list of previous measures of Jupiter and satellites, adding a number of his own made with the 26-inch Washington refractor.

THE next Meeting of the Royal Astronomical Society will be on Friday, January 11; of the British Astronomical Association on January 30.

FROM AN OXFORD NOTE-BOOK.

THE new century is coming quietly upon us without any great flare of trumpets. Several proposals have been made for marking its advent by some special new departure, but most of them have come to nothing. There was at one time (about 1885) a chance that we might inaugurate the twentieth century by a universal time-system; but difficulties came in the way of any united action, and though we seem to be attaining the same end by a gradual process, no particular date marks the transition from the old state of things to the new, any more than in the case of the adoption of the Gregorian Calendar. It was, again, hoped by some that Russia would complete the adoption of this Calendar on January 1; but, unless a surprise is being prepared for us, this reform is not imminent. Some half-dozen years ago Sir David Gill proposed a Conference of Astronomers to consider new methods of planetary observation for the new century; but opinion generally did not favour the idea, and although we are very possibly drifting in the direction he wishes, there will be no ceremonious launching of the new methods. We shall have to make our own little ceremonies, each according to his liking. For my own part I propose to devote the last night of the century to specially complete observations of Eros, in memory of Piazzi's discovery of the first minor planet on the first night. There is nothing very brilliant about the idea, but I cannot think of a better. Very probably the weather will entirely spoil the plan. We have *not* been favoured with good weather for the Eros observations in England, and I hope others have fared better; though there is little danger that we shall not have enough photographs.

THERE will be a good deal of arithmetic in reducing the Eros observations. Would it be possible to have an accurate ephemeris constructed for every *hour*? It would take some labour in construction and printing, but, considering the number of observatories taking part and the work saved at each, I do not think anything would be wasted in the long run. An ephemeris to $0^s{\cdot}001$ in R.A. and to $0''{\cdot}01$ in Declination for every hour would be a great convenience. It need not necessarily be that finally adopted, for the corrections to it would be discussed and tabulated as a smooth continuous curve obtained from a large number of observations. Prof. Millosevich's ephemeris is very accurate, but being only for every 24 hours and to $0''{\cdot}1$, interpolation introduces slight inaccuracies. If a more elaborate ephemeris is calculated, account should be taken of the small constant part of the aberration ($0''{\cdot}3$) which (as Mr. Cowell has pointed out) has been hitherto neglected, for the difference between the centre of the Earth's orbit and that of its hodograph (see the last number of this magazine, p. 448); although the effect of this on the ephemeris would vary very slowly, and the omission of it would scarcely affect the parallax.

How many of our readers have heard of Koreshanity? A certain Dr. Cyrus Teed, *alias* Koresh, declares that the Earth is a hollow sphere, with all the celestial phenomena in the middle; and a magazine is devoted to the expounding of this wonderful doctrine. "Koreshanity maintains that all the visible heavenly bodies—the Sun, Moon, planets, and stars—are *within our atmosphere* near the junction of the great seas of oxygen and hydrogen, and that none of the visible celestial orbs are over 1000 miles from the Earth's concave surface." This is quoted from an article in the above-mentioned magazine, *The Flaming Sword*, headed "Phenomenal Refutation of Modern Astronomy," wherein the writer, Prof. U. G. Morrow, proposes to turn the very observations of astronomers against themselves and prove the above conclusion. He proceeds on the following lines:—

> That persistent cause which makes language has destined that meteors should be called shooting-stars; and it is not by any mere whim nor accident that the showers which fall from the constellation Leo are called Leonids. The very name indicates that they fall from the constellation, and that the radiant

trains of light proceed from the star-sphere itself. . . . . There is not a single star in the constellation Leo that is subject to the slightest parallax; and we will show that the *meteoric radiant presents no parallax* when viewed from widely separated stations of observation. . . . . But do the meteors themselves present no parallax? Certainly: when they fall to within 50 miles of the Earth their parallax is enormous; but they proceed from the radiant-point which has no parallax. . . . . . At the acknowledged velocity of 30 miles per second a meteor falls to the Earth from the radiant in about 30 seconds of time, and from this it is conclusive that the atmosphere is about 900 miles in depth, and also that the distance from the constellation Leo to the Earth is equal to the meteoric flight of 30 seconds.

CASES are not infrequent where the editor of a literary journal is victimized by some one copying out a poem from the works of an eminent man and sending it in as his own, but I had not heard until recently of any scientific crime of this kind. The curious will find in the *Comptes Rendus* for 1894 (cxix. p. 451) a paper by *Vernier*, on the problem of three bodies, which is almost a literal transcript of a paper by *Siacchi* published twenty years earlier in the same journal (*C. R.* lxxviii. p. 110). The plagiarist no doubt thought *Siacchi* might be dead, or might not see the second paper; but he was very much alive, and protested, with the result that Vernier has been heard of no more. But there the two papers stand in print, to show what human nature is capable of. Query, when we get the new subject-index, will it be necessary to compare all papers submitted with their predecessors? The life of a referee will not be a very happy one; but on the whole we shall gain by avoiding mistakes of this kind.

ANSWERS given in examinations are a never-failing source of edification. The following is rather a general tangle; I have not been able to visualize the experiment with the ropes and stakes:—

1. Reasons for the spherical form of the Earth:

(1) On watching for the disappearance of a ship the last thing visible is the hull. This proves that the Earth curves, for if the water had been flat, the last thing visible would have been the masts of the ship.

(2) The phases of the Moon are caused by the Earth's coming between it and the Sun, and therefore casting its shadow on it. This shadow is always of a circular shape.

(3) And if at any place or part of the Earth three stakes are driven into the ground at equal distances from one another, and a rope is stretched horizontally between each stake, and one opposite to it, the middle rope will appear the highest, and the farthest one the lowest.

THE subjoined letter was handed to me by an eminent American astronomer, and is given *literatim*, except for the suppression of one or two names:—

Professor of Astronomy,
University of ——

Belleville, Ill., Feb'y 14/98.

DEAR SIR:

No doubt you will be surprised to receive this letter from one you have never heard of before. I hope you will look upon its contents with seriousness, and not as a huge joke, or written by one with a deranged mind. Knowing you to be connected with the largest telescope in the world, and at the head of

Professors in America, I selected you as the proper person to relate my experience to. This is what I saw: On the night of Feb'y 19–1894, I was standing in the yard of my parents home, at about 10 minutes to 7 o'clock. It was a beautiful night; the sky was clear, and the full Moon looked beautiful. I was looking at the Moon admiring it, when suddenly the figure of a man appeared on the inner left edge of the Moon. He was larger than an ordinary sized man, appearing to be at least 10 feet tall. Directly behind him in single file there appeared, one at a time, about 50 more men, appearing to be younger and about 7 feet tall. As they appeared there were an equal number of young ladies appeared on the inner right edge of the Moon. They all proceeded to march to the center of the Moon, when the first figure, (who seemed to assume the leadership) turned to the right and walked directly from the Earth. The young ladies and gentlemen met as they approached the center of the Moon and followed the leader in couples, each gentleman taking a lady, except the leader. It took them about 2 minutes to disappear from view as they walked from me, their heads disappearing first, then gradually their bodies, and their feet last. The men were of all smooth face; the leader looked like a man of about 35 years of age, and the other ladies and men, (rather girls and boys) looked to be about 18 and 21 years of age. The men were dressed in white colored tights from the waist down, the upper portion of their bodies being covered with a variagated colored blouse waist, and no head cover. The ladies were dressed in beautiful white dresses and no head cover. All their garments were of the finest texture, and both sexes were of fine physique, and had beautiful features. They surely looked like a higher type of humanity than we have on this Earth. None of them carried anything, except the leader who had a rod, similar to a shepherds rod. They did not once turn their heads in the direction of our Earth, so suppose they are ignorant of our existence. I only obtained a side view of their features as they appeared and walked to the center of the Moon. I could see no vegetation or houses of any kind, although the light on the Moon seemed to be more brilliant than even our sun-light. To say I was surprised at what I saw would be putting it mildly. Of course excited as I was, I could not observe and remember every little detail, but have given you the best description herein that I can on paper of what I saw. No doubt you will wonder why I have waited almost 4 years without ever saying anything about what I saw. I have never told anyone of what I saw, as I knew they would only laugh at me, and say I was crazy. I have often thought and intended to write to some one of prominence in the Astronomical Field, and finally selected you. There is no reason why you will not be able to see what I saw, if you will but watch for it through your most powerful telescope, or even with the naked eye. It is a matter of conjecture how often an occurrence of this kind takes place on the Moon. I have never seen any being on the Moon since that night, but have never made it my business to watch for another occurrence of that kind, as I realize that I could never make the world believe it. It must be seen and come from a man of your standing for people to ever believe it. I have never studied Astronomy and all I know of it I learned from articles I have read in News Papers and Periodicals. I have read of your wonderful telescope, and also saw illustrated a photograph of the Moon taken by you. You seem to be inclined to think there is no life on the Moon, while the French Astronomers are inclined to believe the Moon is inhabited. I am not writing you in order to obtain any notoriety or publicity. I request you to keep it from the public. I am a young unmarried man 30 years of age living at home with my parents, and I associate with the very best people here and in St. Louis. I will not sign my own name to this, but will give you a fictitious name and address in order to avoid any publicity, but any correspondence directed accordingly I will receive personally. I wish to ask you one question: How tall (or large) would the people on the Moon have to be to appear to people on this Earth as they appeared to me. Hoping this communication will be the cause of you making a wonderful discovery

I remain in all seriousness

James Bennett.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIV. FEBRUARY, 1901. No. 302.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 January 11.

Mr. E. B. Knobel, *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. Fifty-three presents have been received since the date of the last Meeting. Among these is a copy of the recently published second 10-Year Catalogue of the Royal Observatory, Greenwich; this contains the places of 6892 stars reduced to Epoch 1890, made from observations with the transit-circle in the ten years 1886–96. Prof. Bakhuyzen has presented the Society with 26 maps of Borneo, Sumatra, &c., which will be of great value to observers of the approaching eclipse who go to that region.

Mr. J. C. W. Herschel. This paper, which is communicated by Sir Robert Ball, is an account of observations of the Leonid meteors made at Cambridge Observatory on November 13, 14, and 15 last, and Sir Robert desired me, in reading this paper, to thank those members of the University who helped in making these observations. Two ends were kept in view in arranging the programme—one, to keep a count of the number of Leonids seen in each quarter of an hour; the other, to record their directions: for the latter purpose maps similar to those described by Mr. Hinks in *M. N.*, April 1900, were used. The back of the map was blue, and a light behind it shone through needle-holes representing the stars. The tracks marked on these at the time not only showed the direction and the length of the path, but were in the very best form in which they could be used afterwards to combine them and to find the radiant-point, so there was no accuracy lost by a reproduction. The numbers of Leonids seen each quarter of an hour showed that the shower was weak, with a slight increase about 17^h, on November 13, and a slight outburst next night; but the maximum seemed to be between 12½^h and 13½^h. It has been pointed out that neighbouring showers seem to fluctuate in sympathy with the main shower, and the observations seemed to confirm this. As to the radiant-points, all meteors coming from the region of the Sickle are considered Leonids, and *every one* of these were shown to radiate within small limits from the seven radiant-points enumerated in the paper. They were generally green with green streaks, though some of them were noted as reddish yellow or bluish. On the first day there was one meteor which, although passing only 2° from the first radiant-point, would considerably weaken the character of that point, as shown by the other meteors, if it were included. A lantern-slide showed this clearly. Examination of the fifth radiant-point is another illustration. Seven meteors on the 13th might have been combined to show a radiant-point about 2° higher; but combining with the other night's meteors, four were found to suit the radiant better, and, on the other hand, three meteors on the 15th came in to support admirably the lowered position shown by the remaining three.

The President. It is a great pleasure to the Society to receive a paper with the honoured name of Herschel attached. We know how very closely that name was associated with our Society at its foundation in 1821. The paper which we have listened to by Mr. Herschel is one that raises many conflicting doubts in our minds. We have always been accustomed to think of the radiant of the Leonids as being embraced in a very small area, but it appears from this paper that it extends over a much larger area than we were in the habit of supposing. I would invite any observations.

Captain Noble. I think that Mr. Herschel is to be congratulated upon the success that has attended his observations. My own experience was a very limited one. From 11^h 55^m to 12^h 25^m on the night of the 14th I watched for Leonids, but did not see a single one. Towards the end of the time a brilliant meteor, as bright as Aldebaran, shot from Taurus across Cancer, and right into the radiant of Leo. I felt that that was adding insult to injury, and I went to bed directly.

Mr. Bryan Cookson, reading a paper on the Accuracy of Eye-observations of Meteors and the Determination of their Radiant-points, said that in this paper an attempt had been made to find the probable error of the radiant, and of the recorded beginning and end of meteor-tracks which had been visually observed. So far only Perseids had been made use of, but it would be interesting to apply the method to other strong showers such as Lyrids and Orionids. The method adopted was as follows:—A gnomonic projection of the sphere, with its centre of projection at the assumed radiant, was drawn and the equations of the lines of the meteors referred to rectangular coordinates were found by an easy and quick graphical method. Weights were assigned to the equations on the assumption that, so far as direction of path is concerned, an error in the recorded beginning and end of a meteor's path is equally probable in any direction. The radiant is the point such that the sum of the squares of the perpendiculars from it on to the lines of the meteors' paths is a minimum. This gives then a system of weighted equations with two unknowns to be solved by least squares. Altogether 310 meteors had been so treated: of these 149 were observed by Mr. Denning. The general result was that the probable error of a recorded beginning or end varied from $\pm 0^\circ{\cdot}7$ to $\pm 0^\circ{\cdot}2$ for different observers, whilst the probable error of a radiant deduced from 20 to 30 meteors varied between $\pm 1^\circ{\cdot}0$ and $\pm 0^\circ{\cdot}2$. The high accuracy of Mr. Denning's observations was shown in a striking way. [Blackboard.] But it must be remarked that mixed up with the probable error here found is a possible real diffuseness of the radiant: hence the desirability of discussing other showers. It was concluded that, so far as the present discussion went, eye-observations of meteors were of sufficient accuracy to show the existence of stationary radiants.

Mr. Newall. It seems to me that we have every reason to welcome papers of the nature of the two that have come before us this evening, both of them evidencing, as they do, careful and extremely fair-minded treatment of the question of meteors. It makes one feel that Mr. Denning's long work and exertions in this subject have left a permanent mark on the subject, and that the same spirit is present in both Mr. Herschel's account of the observations of meteors and in Mr. Cookson's account of the treatment of the observations made by Mr. Denning and others. It is a remarkable point that the probable error of Mr. Denning's observations appears, by Mr. Cookson's calculations, to come out as shown on the board, and, as Mr. Cookson says, this may be because the radiant is in late years more nearly a *point*-radiant, and that in earlier years the area of the radiant was greater; but, of course, the fact that the probable error diminishes as years go on may be due to the fact that in spite of Mr. Denning's experience before 1877, his observations have increased in accuracy up to 1898, which is the last year in which observations were given by Mr. Denning for treatment by Mr. Cookson. The fact that they go in a constant direction seems rather to point to the view that practice makes perfect.

Mr. Dyson. I should like to ask a question. I think the point in Mr. Herschel's and in Mr. Cookson's papers is the small probable error of the determination of the radiant. I think a good many would have been inclined to think, especially from the diagram, that all these meteors come from one point, and that they were errors of observation, but I think that Mr. Herschel and Mr. Cookson have established a very clear case for these observations. I should like to ask Mr. Cookson if he can tell us what is the average distance of the several tracks in one of these showers from the deduced radiant. I am not sure whether the figures on the blackboard are the errors of the separate tracks or of the deduced radiant.

Mr. Cookson. The particulars on the board are the tracks, but the error of the radiant is of about the same size.

Mr. Dyson. It seems to me that Mr. Denning must observe with remarkable accuracy, and that Mr. Herschel's observations are comparable with his.

Prof. Turner then gave an explanation on the blackboard, and said: I have had an opportunity of seeing Mr. Cookson's paper, and was much gratified to see the result. It did not altogether surprise me, because I had known of Mr. Denning's confidence in the observations, and I have always felt faith in their accuracy; but it is satisfactory to have this accuracy expressed by Mr. Cookson in mathematical form. I take it this result has an important bearing on that very important fact (which Mr. Denning at least thinks is a fact and which I have every confidence in), that certain radiants are stationary. When eminent astronomers challenge Mr. Denning's ideas about stationary radiants by simply saying that he has mistaken a series of coincidences for a definite law, there was no way of refuting them in definite terms. Mr. Cookson's contribution enables something to be put down definitely on that side. The same may be said about Mr. Herschel's observations, which I had an opportunity of hearing about at the British Association at Bradford.

Mr. Dyson read a paper sent by Mr. Alex Roberts, of Lovedale, South Africa, on the light-curve of S Aræ. This star was discovered to be variable by Mr. Innes, of the Cape Observatory. Mr. Roberts had observed it and deduced a curve of light-variation of which the period was small. This fitted the observations very well, the residuals being less than 0·8 of a magnitude.

The President. This question will, no doubt, be elucidated very much when the large McClean telescope is in order.

Mr. Newall. I should like to point out that there is very little difference between this short variable of 10 hours and those which Prof. Bailey has found in small clusters in his observations at Arequipa. There is a remarkable coincidence in the periods between the variables in these clusters. I think there are about 46 variables which have a period of about 12 hours. This is a period of 11 hours, and the similarity of the curve is very striking.

Prof. Turner then read a paper on the Rotation of the Field of a Siderostat. Quoting the equation given by Cornu in the *Astrophysical Journal* for March 1900, viz.,

$$\tan \tfrac{1}{2}\theta = \mathrm{K} \tan \tfrac{1}{2}u,$$

where θ is the angle made by a line in the field with a fixed line, and u is an angle changing uniformly in the period of one day, he pointed out that this equation occurred in elliptic motion, θ being the true, and u the excentric anomaly, and showed how, by a linkwork arrangement of four bars, one of them could be made to rotate precisely in the manner required if another of them was rotated uniformly. Thus the variable rotation of the field can be compensated exactly by a simple mechanical device. He pointed out further the way in which the case of the coelostat formed the transition from cases of rotation in one direction to those in the reverse direction.

Mr. Maw. There would probably be no difficulty in carrying out Prof. Turner's proposal mechanically. There might be some trouble when the direction of motion is reversed; but supposing the diagram to represent a 24-hour period, the gear suggested might act for something like 11 hours without any difficulty at all.

The President. I think we ought to return our thanks to Prof. Turner for the clear explanation he has given us. We have only a few more papers, and the titles of these will be read. The principal business seems to be to ballot for Fellows, of whom we have 13 up for election to-night.

Mr. Newall. A paper has been received from Mr. H. C. Plummer on a method of reducing occultations of stars by the Moon, together with the reduction of occultations observed on three occasions at the Liverpool Observatory. The paper is more suitable for being read at leisure than for reading here this evening. Two papers have been received from the Royal Observatory, Greenwich, dealing with observations of occultations of stars by the Moon, and observations of the Solar Eclipse, 1900, made with the 28-inch refractor; and there were also papers from M. Flammarion on observations of Jupiter and the rotation of Saturn in 1896–97.

The following papers were announced and partly read:—

H. H. Turner. "On Mechanically Correcting the Rotation of the Field of a Siderostat."

Bryan Cookson. "On the Accuracy of Eye-observations of Meteors, and the Determination of their Radiant-points."

J. C. W. Herschel. "Leonids observed at Cambridge Observatory, 1900 November 13–15."

H. C. Plummer. "On a Method of Reducing Occultations of Stars by the Moon, together with the Reduction of Occultations observed on three occasions at the Liverpool Observatory."

E. M. Antoniadi. "Observations of Saturn made at Juvisy Observatory in 1900."

A. W. Roberts. "The Light-curve of S Aræ."

Royal Observatory, Greenwich. "Observations of Occultations of Stars and Saturn by the Moon made in the Year 1900."

Royal Observatory, Greenwich. Observations of the Solar Eclipse of 1900 May 28 made at the Royal Observatory."

The following gentlemen were elected Fellows of the Society:—

Charles Anthony, Jun., M.Inst.C.E., Casilla 1045, Buenos Aires, Argentine Republic.

Henry Osmund Barnard, Superintendent, Trigonometrical Survey, Ceylon.

A. Y. G. Campbell, I.C.S., Assistant Collector and Magistrate, Board of Revenue, Madras, India.

Charles Davidson, Royal Observatory, Greenwich, S.E.

Frank C. Dumat, Johannesburg, Transvaal, S. Africa.

A. T. Flagg, M.A., Chapel House, Westoe, South Shields.

Walter Heath, M.A., Redcott, Cobham, Surrey.

J. C. W. Herschel, B.A., Lawn Upton, Littlemore, near Oxford.

J. N. Jutsum, Cardiff Nautical Academy, 47 St. Mary Street, Cardiff, South Wales.

Capt. J. W. Martyr, 1 The Glen, South Road, Forest Hill, S.E.

A. E. Moore, B.A., B.Sc., St. John's College, Battersea, S.W.

T. M. Nightingale, B.Sc., 375 Bridgman Street, Bolton, Lancashire.

Richard Welford, Thornfield Villa, Gosforth, near Newcastle-on-Tyne.

The following Candidates were proposed for election as Fellows of the Society:—

R. W. Chapman, M.A., B.C.E., Lecturer on Engineering and Physical Science in the University, Adelaide, South Australia (proposed by Sir C. Todd).

Charles J. Isaacs, Head of the Upper Nautical School, Greenwich, S.E. (proposed by T. Lewis).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the eleventh Session of the British Astronomical Association was held on Wednesday, December 19, at Sion College, *Mr. G. M. Seabroke*, President, in the Chair.

The election by the Council of ten new Members was confirmed.

A paper on the "Fireballs of 1900 October 21," by *Mr. Walter E. Besley*, was read. The writer stated that the evening of October 21 last was distinguished by the appearance of a succession of brilliant meteors, one of which was very widely observed. Accounts of it had come to hand from some 258 sources, it having been seen from no less than 35 English counties, as well as from Wales, the Isle of Man, and Ireland. The writer had attempted to obtain a real path from the average positions assigned by the best observations with this result:—Radiant 300° + 10°; heights, 63 miles over a point 3 miles N. of Leominster to 31 miles over a point 5 miles N. of Wolverhampton; length of path 48 miles. This was in fair agreement with the path deduced by Mr. Denning. A number of observers reported sounds in connection with either its passage or its explosion. Reference was also made to several other fireballs seen on the same night.

Mr. Crommelin said that as the fireball did not seem to have approached at any point within 20 miles of the Earth, the sounds said to have accompanied its passage must have been purely imaginary or at any rate quite unconnected with it. It was certainly a very curious coincidence that there should have been six brilliant meteors on one night, and apparently all from different radiants.

Mr. C. T. Whitmell read a paper on "The Shadows of Jupiter's Satellites," giving a record of observations made by Mr. Scriven Bolton in watching the transit of the shadow of Satellite III. across Jupiter. These observations, the writer continued, completely confirmed his theoretical predictions that when Jupiter was at quadrature, the shadow of Satellite III., if transiting equatorially, would be 6½ times broader at the phase-limb than at the other limb, but that the height would remain constant; and if the transit was not equatorial, the change in breadth would be less, and this would also be the case if the observations were made, not at quadrature, but nearer opposition—at which, of course, the changes vanished. A lantern-slide copy of Mr. Bolton's drawing was thrown on the screen.

Mr. Crommelin said it was very satisfactory to have got such an admirable confirmation of Mr. Whitmell's theory. He wished to call attention to the fact that on 30th June next the Earth would transit across the Sun for Jupiter, and during the transit Satellite I. would cross the disc of Jupiter, so that the satellite would be almost entirely occulting its own shadow; but the shadow, owing to the penumbral fringe, would be bigger than the satellite, so that the shadow would be seen as a dusky ring round the satellite. It would be an interesting phenomenon to look out for and would be visible in this country.

Mr. Maunder read two short papers, which would appear in the Report of the Eclipse of May last. This Report, he said, had been promised by the date of the next Meeting. The first paper, by Mr. and Mrs. Maunder, was entitled "Coronal Extensions on the Photographs of the Eclipse of 1900 May 28." As the pair of photographs to which, in the Indian eclipse, the longest exposure was given showed the coronal streamers to the greatest distance from the Sun, it was resolved to give as long an exposure in the eclipse of May last as its conditions permitted, and exposures of 48 seconds were given, as compared with 20 seconds in 1898. The result, however, was that, so far from recording longer streamers than in 1898, the longest ray photographed was barely half the length of the great ray seen in the Indian eclipse. The cause of the failure was inquired into in some detail, and it was noted that the next eclipse, that of May 18, 1901, should, if weather conditions were favourable, give a specially good opportunity for settling the question. It seemed desirable, therefore, that every variety of exposure should be given, including some for the whole length of time available.

The other note was on "Illumination during the late Eclipse compared with Twilight Illumination." As a result of some observations made after the eclipse of 1898, January 22, in India, six of those who observed the last eclipse made attempts, quite independently, to estimate the time at which the general illumination after sundown corresponded to that during mid-totality. Two of the observations stood out, but the other four were in most remarkable agreement. When the observations were reduced to ascertain the distance below the horizon to which the Sun had sunk at the times recorded, they agreed within one or two tenths of a degree. The results pointed to the 1898 corona not having been quite so bright as that of 1900, and the general illumination corresponded to that of twilight when the Sun was between 6° and 7° below the horizon.

Mr. Whitmell asked if the observations were made the same evening.

Mr. Maunder replied that three of them were made the same evening, two on May 30, and one on June 1.

Mr. Wesley remarked that the comparatively small extension of the corona during the eclipse of 1900 showed that at the different minimum sun-spot periods it did not exactly repeat itself. In 1878 the corona was seen to extend many degrees from the Sun both east and west. It was clear that the corona at one minimum period only very roughly reproduced the conditions of the previous minimum, not only as regards the great extensions, but also with respect to the angles at which the rays were diverted in the equatorial direction. It would be most important to have the eclipse of 1901 photographed with long exposures, to see if there were periodical changes in the coronal extensions.

Mr. Crommelin, speaking with reference to the second note read by Mr. Maunder, regarding twilight illumination, said he rather distrusted the results obtained unless the observers were screened from direct sunlight before totality.

Mr. H. Keatley Moore said the party of observers of which he was a member screened their eyes for a minute and a half or two minutes before totality. Unfortunately their observation was one of the rejected ones. Could it be that after all theirs was the only correct one? He was too modest to say.

The President remarked that, seeing that the eye was such a bad photometer, it seemed surprising that the observations ran so near together.

A paper by *Mr. Stanley Williams*, on "The Double Canals of Mars," was then read. The writer remarked that the concluding portion of M. Antoniadi's "Considerations on the Double Canals of Mars," in Vol. xi., No. 1 of the *Journal*, inclined him to think that their views were not so greatly in disagreement after all. It appeared that M. Antoniadi did admit the reality of the duplicity of the Nilokeras, and also that the "optical theory," as defined in recent discussions, did not seem to account for all the doublings seen by him at Juvisy. That being so, he was inclined to ask why invoke the "optical theory" at all? He concluded by expressing the hope that Mars could be exhaustively observed by members of the Association during the coming opposition.

Mr. Maw exhibited a number of slides which had been presented to the Royal Astronomical Society by Prof. David Todd in illustration of his instrument for automatically exposing photographic plates during a solar eclipse. Mr. Maw reminded the meeting that he had referred to Prof. Todd's work in his presidential address in October last, and the slides shown would serve to supplement the remarks then made. The instruments were used by Prof. Todd for observing the eclipse of May last at Tripoli, and by one of them 83 photographs of the corona were taken in 51½ seconds. They were also to be used in Sumatra next year.

Mr. Wesley thought that Prof. Todd's methods of photographing the corona in rings or zones would produce a succession of bands round the Sun in the picture which would interfere with the effect and confuse details.

Some drawings of Saturn by *Mr. Scriven Bolton* were handed round and discussed, and several slides illustrating star-clusters were shown by *Mr. Henry Ellis* as an indication of the work being done by the Photographic Section.

The meeting was concluded by the reading of reports from the New South Wales and Victoria Branches.

ROYAL METEOROLOGICAL SOCIETY.

The Annual General Meeting of this Society was held on Wednesday evening, January 16, at the Institution of Civil Engineers, Great George Street, Westminster. The President, *Dr. C. Theodore Williams*, was in the Chair.

The Secretary read the Report of the Council, which showed the Society to be in a satisfactory condition, there being an increase of 55 in the number of Fellows over those of the previous year. Reference was made to the celebration of the Society's Jubilee on April 3–4 last, and also to the death of Mr. G. J. Symons, F.R.S., who had, amongst other things, bequeathed to the Society about 2200 volumes and 4000 pamphlets from his valuable library.

Dr. C. Theodore Williams delivered the Presidential Address, taking for his subject "The Climate of Norway and its Factors."

He considered that its meteorology should prove an attractive study for the Society, as having much in common with that of our country, both the Norwegian and the British shores being influenced by the same Gulf-stream, and having their winters and summers tempered by the same equalizing agency. The factors which influenced the climate were:—(1) the insular character of the country; (2) the distribution of the mountain-ranges, which explains to a large extent the rainfall; (3) the waters of the ocean, which, from a variety of circumstances, come into close connection with much of the country, and thus temper extremes of climate; and (4) the Sun, which in this latitude remains in the summer long above the horizon, and in the winter long below it. The Address was illustrated by a large number of lantern-slides of Norwegian scenery, embracing mountains, glaciers, fjords, &c.

The election of Officers and Council for the ensuing year then took place, *Mr. W. H. Dines* being appointed President, and *Dr. C. Theodore Williams* Treasurer.

The Spectrum of the "Flash" *.

In this paper the results are given of a detailed study of a series of spectra photographed with a prismatic camera of 2¼ inches aperture and 36 inches focus.

These photographs, which are described in detail in the paper, include the spectrum of the cusps taken immediately before and after totality, and the complex bright line spectrum of the "flash" obtained at second and third contacts. The spectra of the upper chromosphere, prominences, and corona are shown in the plates exposed during the total phase of the eclipse.

The wave-length determinations in the case of the flash-spectra cover the whole range of spectrum between D‴ and λ 3340 in the extreme ultra-violet, but, owing to the small scale of the images, the results are not considered very trustworthy except in the region more refrangible than K.

Tables are given of all the bright lines measured, with their intensities and the elements with which they have been identified. A separate table is also given of the hydrogen lines, of which at least 28 are shown on one of the plates.

The principal results discussed in the paper are those relating to the constitution of the lower chromosphere and the relation between the bright lines of the flash and the dark lines of the ordinary solar spectrum.

Comparing the wave-lengths of the bright lines with Rowland's tables of the solar lines, the two spectra are found to be in reality very closely related, although, owing to great differences in the relative intensities of the lines due to different elements, they would appear at first sight to have little in common.

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* General results obtained from a detailed examination of spectra photographed at the solar eclipse of January 22, 1898. Abstract of a paper by J. Evershed read at the Royal Society, January 17, 1901.

It is concluded from these results that the flash-spectrum does not really represent the upper, more diffused portion of a stratum of gas which, by its absorption, produces the Fraunhofer dark-line spectrum. The differences in relative intensity are explained as the result of the very unequal heights to which the different elements ascend in the chromosphere; the more diffused gases of low density giving strong emission-lines but weak absorption-lines, whilst low-lying gases of great density give strong absorption-lines but weak lines in the flash-spectrum.

A somewhat unlooked-for result of the investigation is the large proportion of lines in the spectrum of the flash which have been identified with known elements.

The hydrogen spectrum and the part played by hydrogen and helium in the lower chromosphere are also discussed in the paper.

The Problem of the Reversing Layer.

Professor Frost's able discussion * of the flash-and-cusp photographs obtained by him during the North Carolina eclipse of last May is almost decisive of the "reversing-layer" controversy. His available material consisted of six impressions, four taken with a prism-train, two with a concave grating, used direct, without slit or lenses. The spectral region sharply defined in them extended from λ 4050 to λ 4900; so that the range of the investigation was somewhat narrowly limited. Its outcome, however, was precise. Of 382 flash-lines measured and compared with Rowland's Table of solar wave-lengths, no less than 241, or 63 per cent., fell into place as reversals of Fraunhofer lines; and where the definition was best, the percentage rose to seventy.

For the complete representation of the connection between the bright and dark lines, however, it was necessary to look at the negative side of the question; the number of Rowland's lines above a certain standard of strength missing from the plates had to be taken into account. Their enumeration proved confirmatory of what the direct method had indicated. "Hence we reach the very important conclusion," Professor Frost writes, "that at least 60 per cent.—and probably many more—of the stronger dark lines of the solar spectrum are found to be bright in a stratum not exceeding (for the great majority of the lines) 1″, or less than 500 miles in height above the solar photosphere. There is, moreover, no reason in general to suppose that this is not equally true of the fainter lines. Therefore we may regard the existence of a reversing stratum at the base of the chromosphere as fully confirmed by the photographs" (*loc. cit.* p. 345).

* *The Astrophysical Journal*, vol. xii. p. 307, Dec. 1900.

Nor is this inference likely to be seriously compromised by the further extension of enquiry. The indications point rather towards a more closely approximate identification of the flash-spectrum with the Fraunhofer spectrum as the technique of eclipse-photography gradually improves. But the satisfactory localization of absorptive action in the Sun by no means removes all the difficulties connected with it. Enough remain to challenge the keenest curiosity, and almost to baffle hopes of arriving at any feasible explanation of the phenomenon as a whole.

The chromospheric spectrum can be distinguished from the flash-spectrum by the length of the arcs formed by its constituent rays. On plates exposed just before and after totality the variously refrangible light of hydrogen, helium, and calcium fringes 100° to 120° of the limb, while the iron-lines, and hundreds besides, figure as mere hair-streaks of brief illumination. But these extremes are not abruptly separated. Arcs are found of every intermediate length, showing that the reversing-layer—as might have been anticipated—is continuous with the chromosphere. The photographs, at least, supply no facts upon which to found a delimitation-scheme; the lower merge into the upper strata; if conditions become modified with elevation, it must be by fine gradations, not all at once, at the crossing of a fixed boundary.

What is quite clear is that the numerous ingredients of the formation are sorted out on some principle strange to our experience. No correspondence can be traced between the length of the arcs and the atomic weights of the substances originating them. Calcium, as is well known, floats higher than hydrogen; titanium reaches altitudes scarcely inferior; scandium, chromium, strontium, all rise high and shine widely, while substances of greatly inferior density show by an evanescent flash. The case of manganese is thought by Professor Frost to be peculiar. A good many of the unreversed Rowland lines belong to it, and the metal is hence inferred to glow out of sight, in a gaseous stratum close to the photosphere. Nevertheless, it contributes a striking set of ultra-violet rays to the chromospheric spectrum; and these are among the lines which—in Sir Norman Lockyer's phrase—become "enhanced" in passing from the electric arc to the spark *. The incongruity exemplifies the apparently contradictory evidence frequently derived from different radiations of the same solar element. The behaviour of calcium inevitably occurs to the mind. Judging by the length of its "blue" arc at λ 4227, we should assign to the vapour a depth of barely one thousand miles; yet the same material, attenuated to the pitch of giving a two-line spectrum, mounts as much higher as solar explosive forces can carry it.

The absorptive inefficiency of chromospheric hydrogen and helium may be an anomaly of the same order. Four, or possibly

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* Jewell, *The Astrophysical Journal*, vol. xi. p. 243.

five, hydrogen-lines (Hε being masked by the H of calcium) are dark in the Sun; thirty are bright in the chromosphere. Yet every ray from the Sun's disc that reaches our eyes traverses the chromospheric filter. Why does it remain unimpressed by the ultra-violet hydrogen sequence? To reply that the chromospheric gases are not in a condition to absorb might seem like a retrogression to Argan's famous rationale of the soporific properties of opium; yet something more than a verbal shift is really involved. Herr Cantor finds that gases rendered incandescent by electricity fail to exert their due selective stoppage upon light*; and his experiments cannot be ignored in the present connection. The perfect transparency of chromospheric helium is accentuated by the occasional appearance of dark D_3 in the penumbræ of spots. Unusual disturbance seems to be indicated by this peculiar symptom; but the alteration of circumstances through which it develops cannot readily be defined, or even imagined. All that need be said is that it tends, *pro tanto*, to assimilate the Sun to stars of the "Orion" type, in which helium-absorption is characteristically strong.

The localization of the reversing-layer fixes the region of faculæ. Manifest outgrowths from the photosphere, they escape no part of the Fraunhofer absorption. Line for line, their spectrum matches that of the Sun. They are, then, wholly submerged beneath the solar reversing strata. But this situation is irreconcilable with the display of calcium-emissions, such as were photographed in connection with faculæ, by Hale and Deslandres. Plainly, such emissions cannot originate beneath relatively cool and dense strata of the same material. Hence (in part, at least) the distinction set up by M. Deslandres between "facular flames" and ordinary faculæ, the formerly being properly chromospheric, the latter, photospheric formations.

Manifold incongruities affect the measurements of the Sun's rotation from the various solar appendages. Spots, Fraunhofer lines, and faculæ all yield different rates: nor can they be arranged in any satisfactory order of progression. The subject bristles with difficulties and is scarcely yet ripe for discussion. But, if Jewell's rule of acceleration outward hold good, fine discriminations will become necessary between results derived from the various absorptive elements, in correspondence with the heights severally assignable to them in the reversing layer. The velocity given, for instance, by Dunér's pair of iron-lines will have to be carefully compared with the drifting motions indicated by lines of titanium, say, or of magnesium or chromium. Many such enquiries will doubtless be attempted in the immediate future. Each must help, in its measure, to bring the problem of the reversing-layer nearer to a solution. A. M. Clerke.

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* *Annalen der Physik*, 1900, p. 462, quoted by G. F. Fitzgerald, *Nature*, May 3, 1900, p. 7.

The coming Solar Eclipse.

THIS title recurs periodically in our pages, but the subject changes, owing to change in point of view. The last "coming" Eclipse has been the "recent," and is now the "last" eclipse, whilst we are publishing results of the eclipse before last in the present number. The next solar eclipse will be on May 18 (civil). It will be invisible at Greenwich, and the places from which it can be seen as a total eclipse are far distant. The nearest is Mauritius, where totality lasts about 3½ minutes; but at the more distant stations in the Malay Archipelago totality will be longer, about 6 minutes in Sumatra, and the Sun will be higher. As to the weather-chances in Mauritius, Mr. Claxton, the Director of the Royal Alfred Observatory, has tabulated and communicated to us * the observed state of the sky at his observatory on May 18 during the last 15 years; and, judging from the data, he says:—"It will be seen that the probable conditions are decidedly favourable. On only two occasions since 1874 has the sky been overcast at 9 A.M. on the 18th of May, and on 16 occasions it has been less than half covered; on nine occasions it has been practically cloudless. The sunshine records only extend as far back as 1887; from the numbers given it will be seen that there has been constant sunshine, between 8^h and 10^h A.M., six times out of fourteen, and nearly constant sunshine on five other occasions.

"The extremes of temperature are from 71° to 77°, while the figures show that nothing is to be feared from excessive damp, nor are high winds to be expected.

"With regard to the most suitable time for taking Sun photographs, it may be mentioned that at Mauritius the best results are obtained from two to three hours after sunrise, so that totality occurs at nearly the best possible time; nearer to sunrise the atmosphere is still, but hazy, and during the heat of the day the scintillation is intense."

This refers only to the Observatory. At other stations, especially on the higher lands, very different weather-conditions may be expected. Near the seashore, then, the chances of fine weather will be greater than on the highlands, and this, combined with the convenience afforded by a fixed observatory, will detract from the undoubted advantage of observing at higher altitudes, which, it may be remarked, are easily accessible by rail (up to 1800 feet).

The island is too small to cause any material difference in the duration of totality at different stations. The maximum is about $3^m\ 43^s$; at Souillac, a village on the south coast, it is $3^m\ 39^s$; at the observatory, $3^m\ 35^s$; and at Mapou, on the north coast, $3^m\ 32^s$.

From the Royal Observatory, Greenwich, Mr. Maunder will probably go to Mauritius and join forces with Mr. Claxton, a former colleague at Greenwich. The Royal Alfred Observatory in that island is primarily a meteorological observatory, but possesses a six-inch equatorial, a three-inch transit, and a four-inch photoheliograph. With the last of these instruments photographs of the Sun are taken regularly, and are forwarded to the Solar Physics Committee to fill up the gaps in the series of solar photographs taken at the Royal Observatory, Greenwich. It is intended that photographs of the prominences and of the lower corona should be taken with this instrument during the total phase, the image of the Moon having a diameter of nearly 8 inches on the plate. A series of photographs would also be taken of the Sun during the partial phase for the determination of the Moon's place, and of the relative diameters of the Sun and Moon.

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* The table is given in the current number of the B. A. A. Journal.

Mr. Maunder will take with him a four-inch photoheliograph lens fitted with a negative enlarger, so as to take an image of the Moon two inches in diameter. This instrument will be used to get photographs of the corona in general. A four-inch Dallmeyer R. R. lens of 32-inch focus will also be taken, to secure, if possible, photographs of the outer extensions.

Mr. Evershed has also promised to lend his two-inch prismatic camera, which was so pre-eminently succcessful in his hands at the eclipse of 1898 in India. This will be used, as on that occasion, for photographs of the spectrum of the "flash"; but the spectrum of the corona will be the especial object to be secured. This prismatic camera and the three instruments described above will be supplied with light from two coelostats, one of 16 inches diameter, and the other of 12 inches.

An observing party, consisting of Mr. Newall, Mr. Dyson, and Mr. Atkinson, will proceed to Sumatra. Mr. Newall represents the Joint Permanent Eclipse Committee, Mr. Dyson goes as representative from the Royal Observatory, Greenwich, and Mr. Atkinson, who rendered great help to the expedition at Ovar last year, has generously volunteered to undertake the lengthier journey to give similar help on this occasion. The observations projected by Mr. Newall are:—(i) The determination of the rotation of the corona; (ii) By means of a grating to photograph coronal rings corresponding to different lines; (iii) Polariscopic observations similar to those made at Algiers in May 1900. His programme also includes the "flash," and some other of the observations made last year. Mr. Dyson has charge of the Thompson 9-inch object-glass, with magnifier, to obtain photographs on a scale of 4 inches to the Sun's diameter; of a double tube with rapid lenses, to show the coronal streamers and the extension; and of a quartz spectroscope, kindly lent by Capt. Hills for the photography of the spectrum of the corona and of the "flash" in the ultra-violet part of the spectrum.

It is reported that Prof. Barnard, of the Yerkes Observatory, is also going to Sumatra. There is an establishment of Jesuit Fathers at Padang, in that island, and we hear, through Father

Cortie, that certain members of the Calcutta Jesuit Mission will proceed to Padang to observe the eclipse. Their equipment includes: (1) a long-focus lens (33 feet 3 inches), giving a solar image of nearly 4 inches on plates 18 inches square; (2) a photographic telescope, with lens of 4 inches diameter and 5 feet focus; (3) a camera with a Rowland's concave grating of 36 inches focus; (4) a prismatic camera of 33 inches focus. The first and third of these will probably be used with a heliostat, the second and fourth will be turned directly to the Sun and short exposure given.

Universal Time.

In our last number the author of the Oxford Note-Book mentioned the adoption of a uniform time-system as one possible way of inaugurating the new century. So many countries have so far anticipated this by adopting what is called, rather badly it may be thought, zone-time, that for the small remainder to open the new era with a new time-reckoning, as Spain has done, would be of so little moment as to be a quite inadequate commemoration. It may not be out of place to record here the history of the movement.

It is doubtful who first suggested a universal time-system, but Otto Struve, in a private letter to Sir George Airy about the middle of the century, said that he thought the time would come when the whole world would set their clocks by Greenwich. This was scarcely more than a prophetic remark, for it was not till a score of years later, when the partial annihilation of space by the introduction of telegraphy and rapid locomotion had shown the necessity for elimination of difference of time, that any practical steps were taken to this end. It was, in fact, in America, where the length of the railway-systems increased the inconvenience of the use of local time, that a scheme was first proposed, when Prof. C. Dowd, of Saratoga Springs, in 1870 published a pamphlet on hourly meridians, giving essentially the plan followed to-day, except that Washington was taken as the initial meridian. This idea was taken up and discussed by various societies and persons, among them Mr. Sandford Fleming, of the Inter-Colonial Railway, and engineer-in-chief of the Canadian Pacific Railway, who read a paper in 1879 before the Canadian Institute, proposing the use of a universal day, beginning at Greenwich mean noon, *i. e.* corresponding with the Greenwich astronomical day.

The Canadian Institute took care not to allow interest in this scheme to die, and copies of the pamphlet were brought under the notice of the governments of various countries. The question was discussed at the Geographical Congress at Venice in 1881, and again was made a special feature of the proceedings at the Geodetic Conference in Rome in 1883, when it was resolved that it would be convenient to have a universal day of 24 hours to be used side by side with local time. No opinion was expressed as to the initial meridian, but it was suggested that an international conference should be convened to choose one, which should be used by all nations as the zero for time and longitude, and as an incidental detail it was resolved that longitude should be reckoned from 0° to 360°, the eastward direction to be considered positive.

In accordance with the suggestion made at Rome, the Prime Meridian Conference met at Washington in October 1884, members being sent by twenty-seven nations, Prof. J. C. Adams, Capt. Sir F. Evans the Hydrographer, General Strachey (India), and Mr. Sandford Fleming (Canada) representing the British Empire. A practical step had already been taken by the managers of the American railways, thanks to the exertions of W. F. Allen and others, for since November 1883 zone-time had been used in all railway work, the whole continent being divided into zones in which the time kept was respectively 4, 5, 6, 7, or 8 hours slow on Greenwich Time, showing that America at least rose above all consideration of national feeling, and had chosen a meridian to suit the convenience of, probably, the greatest number of people. The chief resolutions of the Conference (but it must be understood that the representatives had no power to bind their governments to action) advocated the adoption of a Universal day, which should not interfere with the use of local or other time; that it should be a mean solar day beginning at mean midnight of the initial meridian, the hours to be counted from zero up to twenty-four, and that the initial meridian should be that of Greenwich. It was further resolved that longitude should always be counted from this meridian in both directions up to 180°, East longitude being plus, and West longitude minus; and the Conference expressed a hope that the astronomical day should begin at mean midnight.

It will thus be seen that there were three distinct proposals:— 1. The change of the astronomical day; 2. The use of a Universal day; 3. The reckoning of the hours from 0 to 24 instead of in two periods of 12 each. The first of these, which entailed a re-arrangement of astronomical ephemerides, was discussed at a special meeting of the Greenwich Board of Visitors, but has not been proceeded with except in the *Annuaire* of the Paris Bureau des Longitudes, which made the change in the ephemeris for 1900. The use of a Universal day has not been adopted except in the modified form of zone-time, which, as above explained, was already in use on the American railways, and this compromise like other compromises, seems to be the most convenient and satisfactory way in which different countries can harmonize their time-systems. It need scarcely be said that the choice of Greenwich as the initial meridian met with some opposition: alternative proposals were made that the meridian of Jerusalem, since it is associated with the cradle of the human race, or an island in the ocean, belonging to no nation, might be taken as the starting-point; but these propositions have not found supporters, and, as has been said, most nations have been content to take Greenwich as their standard.

The following Table gives the names of the countries which have adopted the Universal Time-system, with description of time kept and date of adhesion to the movement:—

	Date of adoption.
Greenwich Time.	
Great Britain.	
Belgium	1892 May.
Holland	1892 May.
Spain	1901 Jan.
1^h fast on Greenwich.	
Germany	1893 April.
Italy	1893 Nov.
Denmark	1894 Jan.
Switzerland	1894 June.
Norway	1895 Jan.
Austria (railways).	
$1\frac{1}{2}^h$ fast on Greenwich.	
Cape Colony	1892.
Orange River Colony	1892.
Transvaal	1892.
2^h fast on Greenwich.	
Natal	1895 Sept.
Turkey (railways).	
Egypt	1900 Oct.
8^h fast on Greenwich.	
West Australia	1895 Feb.
9^h fast on Greenwich.	
Japan	1896.
$9\frac{1}{2}^h$ fast on Greenwich.	
South Australia	1899 May.
10^h fast on Greenwich.	
Victoria	1895 Feb.
New South Wales	1895 Feb.
Queensland	1895 Feb.
11^h fast on Greenwich.	
New Zealand.	

It has been stated above that the United States and Canada is divided into five territories approximately by the meridians $67\frac{1}{2}^\circ$, $82\frac{1}{2}^\circ$, $97\frac{1}{2}^\circ$, $112\frac{1}{2}^\circ$, in which the times kept since 1883 November have been respectively 4^h, 5^h, 6^h, 7^h, 8^h slow on Greenwich, known as Intercolonial (sometimes Maritime), New York, Central, Mountain, Pacific. It is believed that the first of these, the time 4 hours slow on Greenwich, is actually little used, and that the railways up to the eastern coast use New York Time.

It will be noted in the above list that S. Australia differs from the rest of her neighbours by using a time not an integral number of hours fast on that of the prime meridian. In 1895 February, this colony adopted the time 9 hours fast; but the people of Adelaide, after trying it, came to the conclusion that their eastern neighbours had an advantage over them in business, as their hours happened earlier; also that they did not have as much daylight after office-hours as they had when they lived by local time, so as a compromise the time 9½ hours fast on Greenwich was made legal. France is absent from the list, and there seems to be an objection to the use of Greenwich *qua* Greenwich as the prime meridian; but there are hopes, in fact a bill has been passed in the French Chamber of Deputies, but not yet by the Senate, to the effect that the legal time shall be that of the Paris Observatory diminished by $9^m\ 21^s$, so that it may not be long before Greenwich Time is used in France. More remarkable, perhaps, is the case of Ireland, where Dublin Time, 25 minutes slow on Greenwich, is kept on the railways and generally throughout the country. But the telegraph service uses Greenwich Time, and in the small villages in Kerry, where the Atlantic cables come to shore, the large proportion of the population who form the staff of the cable companies live by Greenwich and the rest by Dublin.

The third of the suggestions made at Washington, viz., that the hours of the day should be numbered from 0 to 24, met with some popular appreciation, due no doubt to its novelty. Many persons devised watch-dials to suit the new style, or the old and new combined; enterprising clock-makers set up clocks numbered in the new fashion as advertisements; references, jocose or otherwise, were made to the new system in the public press, and a short poem appeared in *Punch* on the subject; but people in England still prefer to have their watches and clocks numbered from 1 to 12, in spite of the difficulties introduced by the letters A.M. and P.M. It is not so universally, however. The Canadian Pacific Railway on part of the system (west of Lake Superior) use the 24-hour dial, as do some other railways in America. In Italy and, it is believed, in Switzerland the use of the 24-hour clock-face was made legal at the same time as the zone-time. In Belgium special legislation to this end was enacted on 1897 May 1, and at the beginning of this year Spain adopted the system. Even so learned a man as the late Prof. Adams has said that he has missed a train because of the letters A.M. and P.M.; but most persons think that to call hours from 13 to 23 would be to add a new terror to Bradshaw.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Tycho Brahé and Sir George Airy.

GENTLEMEN,—

It may seem somewhat strange to couple these names together, but anniversaries do not unfrequently connect persons much farther apart than these two eminent astronomers. Tycho died in 1601; Airy was born two hundred years afterwards, in 1801. Readers who wish to know all the reasons why and the circumstances under which Tycho left Uraniborg, in the isle of Hveen, where by far the largest part of his work was accomplished, in 1597, will find the full details in Dr. Dreyer's exhaustive volume. Suffice it here that he arrived at Prague in June 1599, his enemy, Reymers Bär, who had laid claim to the invention of the Tychonic system (the disputes about which remind one of those between Newton and Liebnitz respecting the invention of the higher calculus), having disappeared at his approach. Tycho at first established himself at one of the emperor's castles, called Benetky, about twenty-two miles to the north-east of Prague. But in July 1600 he removed to that city and met the emperor there. Kepler, who had already made his personal acquaintance at Benetky, joined him at Prague in October and resided with him there until April 1601, when he went to Gratz on account of his health, but returned to Prague in August. Tycho died on the 25th of October, having requested Kepler to complete the Rudolphine Tables as soon as possible. He added the wish, which Kepler could not conscientiously carry out, that he would demonstrate the theory of the planets according to the Tychonic system, and not by that of Copernicus. Kepler, however, fully appreciated the immense value of Tycho's observations and said that there was no hope of anyone ever making such more accurately, which, Dr. Dreyer remarks, would have been perfectly true if the telescope had not afterwards been invented. The latter adds that the result illustrated the assertion of Archimedes, "Give me a place to stand upon and I will move the Earth." Tycho had given Kepler a place to stand upon and he did move the world. Upon Kepler's laws, developed from Tycho's observations, Newton worked out the true system of the planetary motions.

Forty-five years after Tycho's death the first Astronomer Royal was born; two hundred years after the same event, the seventh. George Biddell Airy's birth took place on the 27th of July, 1801, at Alnwick; but he was removed from there in the following year to Hereford, and to Colchester in 1810, about two years after which commenced his visits to his uncle, Arthur Biddell, at Playford, near Ipswich, whose appreciation of his abilities led to his being sent to Cambridge in 1819. The year in which he took his degree (1823) he travelled in Scotland, and on the way paid a visit to his birthplace at Alnwick. This leads me to mention that, as it is stated in his obituary in the *Monthly Notices of the R. A. S.* (vol. lii. p. 212) that the family came, some generations back, from Kentmere in Westmoreland, I can only find one Airey (*sic*) in the County Directory under that place; and if that was the original form of spelling the name, the last Astronomer Royal may be of the same stock as the late Lord Airey. It was while Airy was at Cambridge that the Observatory there was founded; and he writes in his Autobiography that in 1821 he was introduced by letter to Challis, who succeeded him there after his appointment to Greenwich in 1835. At Herschel's suggestion, Airy went to Dublin in 1827, with some idea of being a candidate for the professorship there, which Brinkley had vacated on his appointment as Bishop of Cloyne, but decided against it. Woodhouse died in December of the same year, and Airy was nominated his successor as Plumian Professor, with which was now united the directorship of the Observatory. At that time Arago was at the head of the Paris Observatory, having succeeded Bouvard in 1811, the same year in which Pond succeeded Maskelyne at Greenwich, whilst Encke had only recently been appointed to that at Berlin.

Yours faithfully,

Blackheath, 1901, Jan. 9. W. T. LYNN.

Planetary Ephemerides and Lunar Observations.

GENTLEMEN,—

The form of planetary ephemerides suggested by Mr. Cowell in your December number would apply equally well to the heliometer observations of the major planets from Mars to Neptune now made regularly at the Cape Observatory (see Sir David Gill's recent annual reports). Will you permit me to indulge in some further notions?

As the ultimate end of the determination of the places of the planets is the comparison of observation with theory, the ideal method would seem to be this:—Refer all the stars of comparison in the zodiacal belt to a fixed ecliptic and equinox of a given date, which would naturally be that of 1900 January 0. Once the longitudes and latitudes of these stars were known, the only changes would be those due to proper motion.

Refer the planets to the same ecliptic and equinox in the planetary tables, which is a simplification.

In the cases of the Sun and of Mercury and Venus, which are not suitable for heliometer measurement, the present form of ephemeris must be retained, but the reduction of the Sun's place to the fixed ecliptic and equinox of 1900 will be quite simple.

Thus, when a planet has been trigonometrically connected with the neighbouring stars whose longitudes and latitudes are known, its own longitude and latitude are as easily obtained as its right ascension and declination, which require further reduction before use. In other words, the ecliptic is the natural plane of reference and the change of co-ordinates to the equator and back again is unnecessary with the heliometric and photographic methods of obtaining planetary positions. It likewise seems useless to add precessions everywhere and finally discard them for all but one point of reference. All that is requisite is to find the departure of the equinox and ecliptic from the fixed equinox and ecliptic, and so throw the change on to one body, the Sun. The translation to a new ecliptic and equinox can conveniently be effected every 100 years.

It is, of course, quite clear that the usual R.A. and Dec. observations of the Sun and clock-stars are still necessary to find the position of the ecliptic and equinox from year to year. But in face of the great exactitude of heliometer observations and for minor planets, the ease of photographic observation, it will be difficult to justify meridian observations of any planets except Mercury and Venus.

The use of fixed longitudes and latitudes is perhaps not so obvious in the case of the Moon, as its natural plane of reference is the movable ecliptic. These ideas are probably more fanciful than practical, but may pave the way for other suggestions. Here I would like to add a word on the value of dark limb occultations of stars for determining the Moon's place. Theoretically their accuracy is about 30 times that of meridian observations. An error of $0^{s}\cdot5$ in the recorded time of an occultation can hardly occur, and it corresponds to an error of $0^{s}\cdot02$ in R.A. I would estimate that the probable error of an occultation, *including personal equation*, is about $0^{s}\cdot2$ and the resulting error in the Moon's R.A. about $0^{s}\cdot007$. In the very worst definition disappearance occultations of stars to the 7th magnitude are easily and accurately observed, whilst the Moon's age is between one and eight days, with a 7-inch telescope. For reappearances in similar phase after full Moon, for 7th mag. read 6th mag., and with good definition between 2^{d} and 7^{d} it is easy to go two magnitudes fainter. From 9^{d} to 19^{d} a spot on the Moon could be referred with the heliometer to neighbouring stars with great accuracy. Thus accurate and homogeneous observations can be made during $\frac{1}{1}\frac{3}{4}$ths of a lunation. The irregularities of the Moon's limb, it is true, are very considerable, but occultations take place at all parts, and with systematic work at several observatories three or four occultations per day should easily be secured, which would pretty well eliminate the effect of irregularity. This error is, on the contrary, more or less systematic in meridian observations, to say nothing of the evil effects of irradiation, which vary with the definition and the personal equation. Occultations could without much difficulty be referred to the ecliptic. As the position of the Moon is not necessarily referred to the Sun as the planets have to be, it seems possible to make a real advance in accuracy in lunar observation.

I am, Gentlemen,
Yours faithfully,
R. T. A. INNES.

Cape of Good Hope,
1900, Dec. 31.

Welsh's Balloon Ascent.

GENTLEMEN,—

In thanking you for your very kindly comment on my recent book, 'By Land and Sky,' I would like to remove any possible suspicion as to my having claimed too much for Mr. Glaisher's telescope or power of vision. I had better give Mr. Glaisher's own words in his 'Travels in the Air,' p. 28:—

"I watched Mr. Welsh's fourth ascent throughout from the roof of the Royal Observatory at Greenwich with a good telescope. The day was fine and the air clear, and I was surprised at the facility with which I could follow every movement of the balloon from its departure to its descent. During the whole time that the balloon was in the air, and while it traversed a course of fifty-seven miles, I never lost sight of it for a moment. I saw it rise from Vauxhall at $2^h\ 22^m$, and descend at $3^h\ 40^m$ at a place which I afterwards learned was near Folkestone."

Coldash, Newbury,
1900, Jan. 12.

Yours faithfully,
JOHN M. BACON.

Changes in the Stellar Heavens.

GENTLEMEN,—

With reference to the letter on p. 456, vol. xxiii. of the *Observatory*, Mr. Gore might be interested to hear that the explanation of the naked-eye double-star formed by 61 and 63 Virginis in Ptolemy's time, by the large proper motion of 61, was enunciated some twenty years ago by M. Camille Flammarion in his valuable work 'Les Étoiles,' Paris, 1882, p. 373, where it is clearly stated:—"L'étoile 61 était anciennement voisine de l'étoile 63, et *elle formait avec elle une étoile double visible à l'œil nu. . . .* Mais elle a depuis abandonné sa compagne, et elle s'enfuit d'un vol rapide vers le sud-ouest." This curious discovery of a proper motion visible to the naked eye is accompanied by a diagram showing the movement of 61 Virginis for 10,000 years.

Juvisy Observatory
(S.-et-O.), France,
1901, Jan. 3.

I remain, Gentlemen,
Very respectfully yours,
E. M. ANTONIADI.

OBSERVATORIES.

HARVARD.—The chief facts in the Fifty-fifth Annual Report of the Director of the Astronomical Observatory of Harvard College for the year ending 1900 September 30 are included in the following extracts:—

In my last report it was shown that the annual income of the Observatory, amounting to nearly $50,000, places it, in this respect, on an equal footing with the principal observatories of the world. Its work, however, can be maintained only if all the income is devoted to current expenses, and this becomes every year more difficult, owing to the continued diminution in the rate of interest on invested property. The additional sum of $200,000 would be required to secure permanently the income of six years ago. When a comparison is made with the buildings and instruments of other observatories the result is still less favourable. The estimated value of the buildings at Cambridge is $52,000, at Arequipa $12,000; of the instruments at Cambridge $20,000, and at Arequipa $50,000. About half a million dollars have been expended on the buildings and instruments of the Lick Observatory, and an equal sum at the Yerkes Observatory. Great additions have been made within the last few years to buildings and instruments at all the principal national observatories of the world. The main building at Cambridge is of wood, and is more than half a century old. It contains one of the finest astronomical libraries in the world, and this is in constant danger of destruction by fire.

East Equatorial.—The observations with this instrument have been made by Professor O. C. Wendell, and have been of the same general character as in previous years. Twenty-four thousand photometric light comparisons, mostly of variable stars, have been made, principally with the polarizing photometer with achromatic prisms, described in the annual report for the year ending September 30, 1895. The same instrument has been used in the photometric measurement of Jupiter's satellites while undergoing eclipse. 18 eclipses have been observed, making the total number 702. Iapetus, the outer satellite of Saturn, was measured on 15 nights, to determine its variation in light, and Titan, also, on 15 nights.

The number of photographs taken with the 11-inch Draper telescope is 689, making 12,872 in all with this instrument; with the 8-inch Draper telescope 2187, making the total number 25,890. The examination, by Mrs. Fleming, of the spectra contained on these plates, and of those taken with the Bruce and Bache telescopes, has led to the discovery of fourteen new variable stars, ten of which were detected from the presence of bright hydrogen lines in their spectra.

The Bruce telescope has remained, during the year, under the special care of Dr. Stewart. Work has generally been carried on with this instrument, however, during the last part of the night, by Mr. W. B. Clymer or Mr. R. H. Frost. During the year 760 plates have been taken, making 4767 in all. Many photographs of the planets and other special objects have been made by Dr. Stewart. From an examination of 319 plates he has found, in addition to those already announced, 198 new faint nebulæ. On these plates were also noted 92 asteroid trails and 2 meteor trails. On one photograph no less than 7 trails of asteroids are visible. Last spring a successful attempt was made to obtain photographs of the planet Eros when it was too faint to be observed in other ways.

12-inch Meridian Photometer.—With this instrument 79,024 settings have been made by the Director on 140 nights. The first large piece of work, the observation of all the stars contained in the Durchmusterung, in zones 10′ wide and 10° apart, is substantially completed. The total number of stars is 9233, of which 3038 are contained in Volume XXIV., and the others are fainter than the ninth magnitude.

Eros.—The near approach of the planet Eros to the Earth is generally regarded as the most important astronomical event of the year. The value of observations of this object for parallax is perhaps over-estimated, since it will be too faint for satisfactory measurement with heliometers, and it is doubtful if our present knowledge of the solar parallax can be greatly improved by observations of Eros made this year. Accurate determinations of its absolute positions will doubtless be of great value, but this work will be undertaken at so many observatories that much of the time spent could probably be more usefully expended on other objects. Accordingly, while many photographs will be taken here, it is doubtful if all will be measured, unless the number obtained elsewhere proves to be insufficient. The exposures here are made automatically, moving the plate after each. Ten images are thus obtained on each photograph. To determine the parallax, additional photographs will be taken east and west of the meridian, with films reversed. The most important observations of Eros that will be made here will be those relating to its variation in light. This will be found from the photographs, and also from visual observations. Ten stars have been selected from the region traversed twice by Eros, and it is compared with them every clear evening. Eros is also compared with adjacent stars by means of the photometer with achromatic prisms attached to the 15-inch Equatorial.

PUBLICATIONS.

THE GÖTTINGEN HELIOMETER MEASURES.—Dr. Schur has earned a reputation for painstaking and accurate observation. No investigation, however long and wearisome, seems to him out of place if by its aid he can satisfy himself of the accuracy of the last decimal place. His monograph on the Præsepe cluster distinctly added to that reputation, and those who admire elaborate work, carried throughout all its details with the utmost care, will find much to approve in his latest contribution, wherein he discusses the relative positions of some fifteen stars in the Perseus clusters. Many discussions within comparatively recent times have been made of the relative positions of the brighter stars in the well-known contiguous groups *h* and χ Persei, to which filar micrometers and heliometers and photographs, and even meridian observations, have contributed. Presumably the final accuracy that characterizes these several attempts is very various. But Dr. Schur appears to have been mainly influenced by that one investigation which, from its nature, has probably the least claim to final accuracy. Mr. O. A. L. Pihl was an amateur astronomer engaged in business in Christiania, but being in possession of a parallactically-mounted refractor of 81 mm. aperture, and provided with a bar micrometer of the Boguslawski pattern, he courageously attacked the measurement of the stars in the cluster χ Persei. As a matter of fact, he did very well with his means, but whether they were of that delicate description as to make their employment in the special direction in which they were used judicious is another matter. He determined the positions of some 236 stars varying in magnitude from 6·6 to 11·6, and reduced the whole to the equinox of 1870. The publication of Vogel's treatise in 1878 made Mr. Pihl aware of some of the sources of error which affected his observations, and he did his utmost to rectify them, probably not very successfully, since Dr. Schur deliberately declares that "Veranlasst durch die grossen individuellen Unregelmässigkeiten, die sich bei der Vermessung von Pihl zeigten, beschloss ich eine Anzahl von Sternen aus beiden Sternhaufen durch eine möglichst Sorgfältige Triangulation mit zahlreichen überschüssigen Beobachtungen untereinander zu verbinden." Such a statement is well calculated to make an amateur pause before he undertakes a piece of work which, tested by the refinements of the most delicate machinery of the day, may be found wanting. Whether such a result is desirable or not we do not propose to discuss here. Much might be written on either side, but the immediate effect in this case was to induce a Director to employ the resources of the Observatory for some five years in deriving the positions of fifteen stars. The observations began on 1891, October 13, and were concluded on 1896, February 17. No doubt other work was carried on at times, when it was impossible or inconvenient to observe the cluster. Indeed, we have a subsidiary investigation into the position of eight stars in Leo, which established conclusively the very interesting result that the measure of distance from end to end of a long arc does not coincide with the sums of the shorter distances measured between the stars occupying intermediate positions in that arc. But this was a necessary enquiry for the rigorous reduction of the Perseus stars, and the results are incorporated in the discussion.

One is tempted to ask, what are the proper uses and functions of a heliometer, and is one justified in occupying more than five years in deriving the distances between some fifteen stars? If this length of time be necessary, there are some who will think that the instrument stands condemned by the slowness with which it wins its results, and the fatiguing character of the reductions. On the other hand, there are those who are so wedded to the accuracy of the final decimal place, so convinced of the necessity of extreme rigour in observation and reduction, that they will glory in the minute care displayed on every page of the Göttingen memoir. But although we feel compelled to place on record our testimony to Dr. Schur's skill and patience, we are unconvinced that the expenditure of so much time and labour is well bestowed, and in support of that contention one or two further facts may be added. The heliometer does not readily supply the means of passing from measurements of distance to differences of Right Ascension and Declination. There are difficulties to be encountered in the determination of the zero of the position-angle, which, if not satisfactorily met, are liable to seriously impair the value of the earlier work. It is needless to say that Dr. Schur has bestowed on this part of the work the same minute attention which characterizes the whole, and fearing that the Göttingen transit-circle was not equal to the accuracy demanded, he applied to the Director of the Berlin Observatory for the positions of certain stars. Ultimately the whole of the fifteen stars were observed on two nights over the meridian of Berlin, and the following table exhibits the difference between the final Göttingen results and those obtained at that Observatory. There are certainly fifty night

Star B.D. +56°.	Berlin — Göttingen.		Star B.D. +56°.	Berlin — Göttingen.	
	da.	*dδ.*		*da.*	*dδ.*
	s	″		s	″
471	−0·02	−0·2	555	0·00	0·0
479	+0·04	0·0	567	−0·01	−0·2
498	+0·03	−0·1	568	−0·07	−0·3
500	+0·02	0·0	593	−0·01	−0·1
530	+0·02	−0·2	595	−0·01	−0·2
543	+0·03	+0·4	598	−0·01	+0·5
545	0·00	+0·5	608	+0·02	−0·1
547	−0·05	0·0			

observations combined to give the heliometrical results against the two at Berlin, and the labour of reduction in the two instances can safely be left to the judgment of those who are familiar with the working of both instruments.

There is a certain progression in the character of the differences which might be removed by a shift in the position-angle of the normal line to which the Göttingen positions are referred. Whether the method adopted by Dr. Schur is altogether free from objection is a point on which only an expert is qualified to express an opinion. We are simply concerned to point out that results, comparable in accuracy, are obtainable with the transit-circle with not a tithe of the labour and time required by the elaborate employment of the heliometer. William E. Plummer.

Verzeichniss von 336 Sterncatalogen *.—This is a general synopsis of all published meridian catalogues from 1750 to 1900. In all there are 336 catalogues tabulated by M. Ristenpart, who is to be thanked for supplying so real a want, for since Mr. Knobel's summary in 1877 nothing of this kind has been done. The table consists of six columns, and enables one to see at a glance the author, the symbol generally used, the number of stars in the catalogue, a rough classification of the stars, epoch, and the more elaborate title. For example:—

Astronomische Gesellschaft, IX. Stück.	A. G. Camb. E.	14441	24° 15' bis 30° 57'	1875	Cambridge, England, 1897.
Kasan.	Ka	202	+54° 30' bis 55° 47'	1885	Kasan Zenith Stars, pub. Kasan Observatory (in Russian), 1893.

It is thus seen what a valuable aid it forms to all interested in these catalogues. Without actually working with such a list it is impossible to say much as to its completeness; there are, however, two points which strike one as requiring explanation. The B. A. C. is not included—possibly because it is not an original catalogue; still it is a catalogue. The Nice observations are also included, which seem to us should scarcely be included in a list of *catalogues*, as they are published in the form of daily journals of observations.

NOTES.

Comet Notes.—The Comet 1900 *c* (Giacobini) appears to belong to Jupiter's comet family. Prof. Kreutz has deduced the following elliptical elements from observations on Dec. 24, 28, Jan. 14 (*Ast. Nach.* 3682):—

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* 'Verzeichniss von 336 Sterncatalogen,' summengestelt von Fr. Ristenpart. Breslau, 1901.

Epoch	1901 Jan. 14·5 Berlin M.T.	
M	6° 45′ 47″	
ω	171 29 11	1901·0.
☊	196 32 34	
i	29 52 17	
ϕ	47 52 36	
μ	515″·91	
log a	0·55829	
Period	6·878 years.	

Perihelion Passage 1900 Nov. 28·29 Berlin M.T.

The orbit resembles those of Wolf's Comet and Comet 1892 V. (Barnard).

Ephemeris for Berlin midnight.

	R.A. h m s	S. Dec. ° ′		R.A. h m s	S. Dec. ° ′
Feb. 2....	2 22 4	18 12	Feb. 26....	3 44 40	11 57
10....	2 52 10	16 6	Mar. 6....	4 7 53	10 1
18....	3 19 33	14 0			

The comet is now extremely faint. A. C. D. C.

MINOR PLANET NOTES.—Five new planets have been discovered, all by Prof. Wolf and his assistants at Heidelberg, viz.:—FV on Jan. 9, FW on Jan. 13, FX on Jan 17, FY on Jan. 18, FZ on Jan. 18. They are all of the 12th or 13th magnitude.

There was also an announcement that Mr. Brooks, of Geneva, U.S.A., had discovered three minor planets near Eros; this, however, proved to be a mistake. A. C. D. C.

DOUBLE STARS.—In the *A. N.* 3680–1 Dr. Doberck gives a set of measures of double stars made while staying with Prof. Thiele at Copenhagen. We are surprised that so experienced an observer should enter his stars under all and any name but the one by which it is generally known. Being also a computer of orbits he must know that he is giving needless trouble to those who collect the measures. For instance, before the measures of H. IV. 88 could be entered, reference had to be made to a general catalogue, where it was found to be no other than Σ 412 A.C., or 7 Tauri A.C.

Some of Dr. Doberck's remarks on the instrument are of great interest. The object-glass is 14 inches, the focal length 15½ feet. The images of the stars are much fainter than in other refractors of smaller aperture through which he has looked, which he attributes partly to the situation being in the midst of a large town, and partly to the great convexity of the lenses both in the O.G. and the eyepiece, and suggests that a remedy might be in the use of single lenses in the eyepiece. Short focal length is a drawback to double-star work by reason of the difficulty in focussing, because the diameters of the wires are proportionately increased, and because much stronger illumination of the field is required. He suggests quartz fibre for short-focus telescopes.

The instrument was installed in 1894. The dome is moved by a dynamo, which can turn it completely round in 8 minutes.

Prof. Hussey publishes, in No. 76 of the 'Publications of the Astronomical Society of the Pacific,' a discussion of the measures of δ Equulei [OΣ 535], with the view of calling attention to the necessity of observing it very frequently. The orbit of δ Equulei belongs to that class in which the measures may be changed through 180° by reason that the components are of very similar magnitude. It follows that by so placing the measures the period can be made a sub-multiple of the longest period which can be assumed.

By arranging all the measures in the first and fourth quadrant, Prof. Hussey endeavours to show that, instead of the accepted period, 11·6 years, it is more likely 5·8. Much as we should like this small period, we are constrained to admit that the present paper goes no further than Otto Struve in 1874. δ Equulei was discovered as a close double by Otto Struve in 1852, who observed it frequently up to 1874. In vol. ix. of the 'Poulkova Observations' he remarks that he could not satisfy himself as to the period, but leaned towards the smaller. Admitting, for the moment, Prof. Hussey's arrangement of observations, we would rather favour a period of $\frac{11\cdot6}{3}$, or 3·9 years. This only shows the need of following the pair closely, and Prof. Hussey is to be thanked for drawing attention to it.

The same number contains an orbit of 99 Herculis, by Prof. Aitken, who obtains a period of 63·0 years. It is a difficult star, and it is impossible to satisfy all the measures. The following are the computed positions :—

1901·5	323°·2	1″·38
1902·5	325 ·0	1 ·43

NEW DIRECTOR OF THE LICK OBSERVATORY.—The vacancy at Lick, caused by the death of the Director, Prof. J. E. Keeler, has been filled by the appointment of Mr. W. W. Campbell. When Prof. Keeler left Lick, in the year 1891, to take up the Directorship of the Allegheny Observatory, Mr. W. W. Campbell, who had been Instructor in Astronomy in the University of Michigan, was appointed to fill the vacancy, and has remained a member of the staff on Mount Hamilton ever since, and now takes his place as its head, as a fitting reward for ten years' solid work. Prof. Campbell's observing, in his earlier years at Lick, comprised such subjects as photographs of Jupiter, Nova Aurigæ, the spectra of comets, and diameter of Mars; but of late years he appears to have devoted himself more particularly to spectroscopy, and determination of stellar motion in the line of sight; his researches of this kind on Polaris will be remembered, as well as his detection of the binary character of Capella. We offer congratulations to Prof. Campbell on his appointment, and our wishes for his long tenure of the office.

MESSAGES FROM MARS.—We feel that some apology is needed for the appearance, in these pages, of a note with this sensational heading; but there have been so many paragraphs lately in the daily press under the title, that on the sole ground that this is a chronicle of astronomical and *quasi*-astronomical events, we think the subject should be mentioned. First, a telegram came from America saying that Mr. Douglass, at Lowell, had seen a projection on the northern edge of Icarium Mare which remained visible for more than an hour. Some one interpreted this by the well-worn surmise that the Martian inhabitants were signalling to us. Sir Robert Ball tried to convince the public of the fallacy of this in one of his lectures at the Royal Institution, and M. Flammarion, with the same end in view, explained to an interviewer that this appearance was probably a sunset effect on our neighbour. Next, Mr. Nikola Tesla, having a wireless telegraphy apparatus on a mountain in America, said that he had noticed effects on his receiver for which he could not account, and therefore he concluded that they must have been caused by the inhabitants of Mars. The message, according to Mr. Tesla, was not particularly lucid, being, as he said, merely "One, two, three"; but he did not hesitate to explain that it is quite within the bounds of possibility to establish electrical communication with our neighbour. These utterances, which have been cabled at length to the English papers, have furnished texts for many jocose paragraphs and small poems; the interviewers have been busy, and Mr. Tesla has got the notoriety he no doubt wanted. M. Loewy very naturally asked his interviewer, "Why Mars? How did Mr. Tesla know that it was not Venus or Mercury who was signalling"?

A third matter, which comes under the heading, relates to a bequest of a Madame Guzmann, who, in 1891, left 100,000 francs to be awarded as a prize to the first person who shall be successful in communicating with another world *other than the planet Mars*. The Academy hesitated to accept the bequest; but has, at last, done so, influenced, perhaps, by the fact that the will goes on to say that each time the prize has not been awarded for a period of five years, the accumulated interest shall be attributed to a work seriously helping the progress of astronomy. The Academy, in accepting the trust, remarks that the intentions of the founder will be scrupulously executed, and quotes from Montaigne:—"It is a stupid presumption to condemn as false all that which may not appear likely to us. There is no greater madness in the world than to reduce everything to the measure of our capacity and competence." But why is Mars excluded? Is it to bar Mr. Tesla?

THE Paris Academy has awarded prizes as follows:—The Lalande Prize to M. Giacobini, for his cometary discoveries; the Damoiseau Prize to Prof. Hepperger, of Graz, for his investigations relating to Biela's Comet; the Valz Prize to the Abbé Verschaffel, for his invention of a new chronograph and other work; the Janssen Prize to Prof. Barnard, for his discovery of the fifth satellite of Jupiter.

UNIVERSAL TIME.—At midnight on 1900 December 31 Spain adopted Greenwich Time as the legal time of the country, the hours being numbered from one to twenty-four.

THE WEATHER IN JANUARY.—Cold weather prevailed till the 9th, the mean temperature for these days being 33°·1, or 5° below the average. On the 7th the defect of mean temperature amounted to 10°·8. The lowest recorded temperature was 21°·0 on the 9th, and on the 7th and 8th the minimum temperatures were 23°·6 and 23°·3 respectively. From the 10th to the 27th (inclusive) the weather was generally mild, the mean temperature being 42°·3, or nearly 4° in excess of the average value. There were no days of exceptionally high temperature, the highest having been 54° on the 27th, a value which has been frequently exceeded in former years. At times very high barometric pressure was recorded, notably about the 4th, the 13th, and the 23rd. On the 27th a violent westerly gale was experienced, and the weather afterwards became much colder. Rain fell on 11 days in the month, amounting to 0·76 inch, an exceptionally small amount, falling short of the average fall by 1·23 inch. W. C. N.

ERRATA IN THE 'COMPANION,' 1901.—p. 8, Jupiter, col. 7, transpose "m" "h"; p. 23, in col. 2 of Table, the sign of P should be reversed in all cases; p. 29, period of Algol, *for* "h m s" *read* "d h m."

WE much regret to hear of the death of Mr. Basil Woodd Smith, J.P., of Hampstead, one of the oldest members of the Royal Astronomical Society and a frequent attendant at its Meetings.

THE magnetic elements given on p. 68 of our January number are for Greenwich.

We have been favoured with a copy of Prof. Turner's new book, 'Modern Astronomy,' which we hope to say more about in our next number.

The Annual General Meeting of the Royal Astronomical Society will take place on Friday, February 8, at 3 o'clock in the afternoon. The proposal is to be made, and there will probably be a discussion on the question, that the Ordinary Meetings shall in future take place at 5 o'clock. The next Meeting of the British Astronomical Association will be on February 27.

In view of the re-measurement of the arc of the meridian in Peru, M. F. Gonnessiat, of the Lyons Observatory, has been appointed Director of the Observatory at Quito for five years.

The Gold Medal of the Royal Astronomical Society has been awarded to Prof. E. C. Pickering, of Harvard.

From an Oxford Note-Book.

The most notable event in Oxford during the past month has been the Proclamation of Edward VII. as King. It is not fitting to speak here of the death of our beloved Queen; but it may interest others to know how a new King is proclaimed. We were summoned to the Divinity School (the big assembly hall of the University) at noon on Friday, January 25. The large hall was naturally crowded. Punctually at noon the Vice-Chancellor appeared and read in Latin the reason of the meeting—he had received a message from high quarters. The Senior Proctor read the first part of the message, which enjoined the reading of the Proclamation. This was done by the Vice-Chancellor, and as he read we could hear the distant roar of the crowd at Carfax *, where the same ceremony had been finished apparently a few seconds earlier. The Vice-Chancellor ended with an impressive "God save the King!" and the assembled Heads of Houses, Doctors and Masters of Arts lustily sang the National Anthem in its new form, followed by "Three cheers for the King!" (I have never before heard a Vice-Chancellor call for "three cheers"). Then a procession was formed to the University Church, which was reached after running the gauntlet of a fire of kodaks. Passing right through the church the representatives of the University assembled on a platform in front of it in time to receive the Mayor and City authorities from Carfax, who again read us the Proclamation. The Volunteers meanwhile were having a very hard struggle indeed with the crowd, which simply packed the High Street and pressed

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* "Quatre-voies," the cross-roads at the centre of the city; called Fourways in Thomas Hardy's novels.

with much unseemliness. This ended the ceremony for the University; but the City authorities went on and read the Proclamation again on the four spots where the City gates once stood. I believe the whole programme was an exact reproduction of the observances sixty-three years ago.

THE closing of a century and the opening of another brought inevitable reviews and stock-takings of all kinds, and science came in for a considerable share of attention. Sir Edwin Arnold, in his manifesto in the *Daily Telegraph*, speaks quite encouragingly of science, and selects certain names for special commendation:—

> Star-eyed science has by no means been asleep during the era so nobly started by Dalton and Davy in chemistry, by Lyell in geology, and such truly great workers in physics as Darwin, Huxley, Tyndall, and Lord Kelvin.

But he has a complaint to make against astronomers. They have not sufficient imagination, and do not realize their true mission in the world. Surely Sir Edwin must have neglected the writings of M. Flammarion or he would not write as follows:—

> Yet is it strange how philosophy and religion still seem, in an age illuminated by Galileo, Copernicus, and Newton, to stick fast in the unbeliefs and narrow estimates of Death and Destiny which were natural for Ptolemy and Dante. No astronomer has dared hitherto to let the teachings of his spectroscope and stellar chart inspire him to adequately rebuke the petty theories of some pulpits and the materialists. People of capacity still go about saying that there can be no life on the Moon because water does not exist there, and that the Sun must roll in his glory untenanted because a Jones and a Smith cannot breathe incandescent hydrogen.

MR. NIKOLA TESLA has been saying things about communicating with Mars, some of which, as reported in the papers, sound rather wild; but it is difficult to make allowance for the amount of dressing-up done by the reporter, and I prefer to wait until some formal statement is made by him before deciding whether he is serious or not. Those who heard him lecture at the Royal Institution some years ago will be ready to believe that if he has anything to say it will be worth hearing. Sir Robert Ball, with his wonderful facility as a popular lecturer, amused his audience at the Royal Institution by a reference to this exciting topic. He reminded them that if the signalling to Mars was to be done by flags they would have to wave a flag as big as Ireland. But supposing any means, visual or electrical or otherwise, were found for communication, he felt sure that when that interesting event came about the substance of the first message received would be contained in three words. And at this he paused, while his listeners waited breathlessly for further enlightenment. "Those three words would be," said Sir Robert, "'Are you there?'"

MR. H. G. WELLS is writing a fine story in the *Strand Magazine* called "The First Men in the Moon." He imagines the discovery of a substance ("Cavorite") opaque to gravitation, with which his two principal characters surround a sphere containing themselves and all their belongings. The cavorite covering is not permanent, but is in sections, each of which can be rolled up like a roller-blind. When all the blinds are shut no gravitation will affect the inside of the sphere, and it will fly on through space in a straight line. Open a window and any star or planet in that direction will attract it, so that the occupants are enabled to tack about in space just as they wish. They voyage to the Moon and land on the dark side, to find it very cold indeed, too cold to think of venturing out of the sphere. But the sunrise brings with it a volatilization of air which had been lying frozen, so that they come out and breathe and have adventures. They are surprised to find that the ordinary effort of taking a step carries them like an enormous jump, and the vegetation springs up under the scorching rays of the Sun at a stupendous pace. The tantalizing "To be continued" occurs in the last number just at the point where they have been jumping about with such glee and recklessness, and the vegetation has been springing up so fast, that they have lost their sphere, their link with civilization; and we look for exciting events in the sequel. It would be ungracious to criticize so charming a romance, but one cannot help feeling an occasional surprise that an author whose scientific knowledge is evidently so complete and modern should occasionally make obvious mistakes. Perhaps it would be impossible to write the romance without them.

THE same number of the *Strand Magazine* which leaves us so anxious for the fate of these new Jules Vernians does its best to allay our anxieties as to what the coming century is to bring forth. Eight men of science have been interviewed by Mr. Frederick Dolman on the subject, and give characteristic replies to his enquiries. Sir Norman Lockyer thinks we shall be able to predict and possibly avert famines in India by study of sun-spots; and I see that he and his son have been reading a paper to the Royal Society on the subject. To me the same old mistake seems to be made in this paper—of generalizing from data extending over far too short a period. But Sir Norman has had some experience, and ought to know what he is about by this time. Perhaps he does. Sir William Preece votes for "the unexpected" happening, and adroitly evades the interviewer in this way. Sir J. Wolfe Barry says that, whatever happens, the engineer will be to the fore; and is inclined to predict the general adoption of the moving platform at the Paris Exhibition (in which suggestion, however, Mr. H. G. Wells has anticipated him in 'When the

Sleeper Wakes'). Sir William Crookes has ideas of the same kind, and also hedges on "the unexpected." Mr. Swan looks for some new chemical source for electricity, and M. Berthelot for chemical food. Sir Henry Roscoe says we shall witness the harnessing of many Niagaras, and improved sanitation; and Mr. Thomas Bryant hopes for means of preventing or curing many more diseases, and the proper use of hypnotism. It is a most interesting series of predictions, and we ought to be very grateful to Mr. Dolman for his enterprise.

THE Scientific Results of the Norwegian North Polar Expedition are being published in English (Longmans, Green, & Co.) in a series of beautifully printed quarto volumes, of which Vol. VI., which has recently appeared, contains the Astronomical Observations edited by Prof. Geelmuyden. It is wonderful what a lot of observing the companions of Nansen managed to accomplish in their trying conditions, and what good observations they made. Their best instruments were a 2-inch altazimuth, with circles of 4 inches radius, and a 3-inch telescope, with an eyepiece of power 65 used for observing Jupiter's satellites; and with these and smaller instruments they did excellent work. Prof. Geelmuyden finds some difficulties in making the best use of the 80 observations of Jupiter's satellites, because of the lack of good tables, and of corresponding observations elsewhere for check. He has discussed methods of empirically correcting the observations after the manner of de Glasenapp, and it is interesting to note that he knows this work through the pages of this magazine. "By the courtesy of the author I am in possession of the original memoir, but as I am quite unacquainted with the Russian language, my knowledge of its contents rests on a very clear abstract given by Dr. Downing in *The Observatory*, Vol. XII." It is no doubt selfish, but one cannot help feeling some gratification in the fact that English should be the means of communication between Russia and Norway, and the Norwegians have thus paid us the double compliment of reading Russian in an English translation, and printing their own work directly in English for the Russians to read if they be so disposed.

IT is good news that Prof. Barnard is going to Sumatra to observe the Eclipse next May, taking the 61-foot telescope and coelostat. He goes with a party from the Washington Observatory, westwards, unfortunately, so that we shall not see him in Europe on his way. Mr. Newall and Mr. Dyson are going out on a Dutch boat in company with Professors A. A. Nÿland of

Utrecht, W. H. Julius (Professor of Physics), and W. H. Wilterdink (Second Assistant at Leiden). The Dutch have supplied valuable information and assistance already, and are obviously prepared to be most hospitable in Sumatra. The writer of these Notes, who has had occasion to mention the incidents of several Expeditions in the last few years, feels very sad at not being able to go this time.

THE following came on a post-card from a correspondent. I must apologize for his language; it is apparently due to the stress of feeling occasioned by getting out of one's bed at 3 A.M. on a cold morning to take a photograph of Eros, (half?) hoping that it may be cloudy. I have known the feeling, but I did not know the proper Greek line to quote:—

> I send you an astronomical quotation from Homer, Iliad iii. 364. Perhaps you will think it good enough for the Oxford Note-Book:
>
> 'Ατρείδης δ' ῴμωξεν ἰδὼν εἰς οὐρανὸν εὐρύν.
>
> "And the son of Atreus said 'Damn' when he saw that the sky was clear."
>
> *ῴμωξεν*=said *οἴμοι* or "alas," but I have tried to be idiomatic. I think it is legitimate to translate *εὐρύν*, broad, as "clear," for the sky only looks broad when it is clear. Notice the emphatic position of the word at the end of the line.

THE following is from the *Daily News* of 1900 December 12. Comment is superfluous:—

> Our Berlin Correspondent's telegram to-day refers to the quarrel between General Chaffee and Count von Waldersee regarding the removal of the famous bronze astronomical instruments from the Tartar Wall at Peking. These instruments were manufactured in the seventeenth century under the direction of Ferdinando Verbiest, the Jesuit Father, who was President of the Board of Works at the Court of the Manchu Emperor, Kang Hsi. They consist of a sextant, a quadrant, an armillary sphere, a great celestial globe adorned with gilt constellations, and a few other instruments, one of which, an azimuth dial, was presented by Louis XIV., "le Grand," to the Chinese Sovereign. These scientific appliances have long been a conspicuous and familiar object to travellers, and, during the last two or three decades, a favourite mark for photographers. Erected under the open sky on an elevated bastion, rising above the summit of the East Tartar Wall, the bronze fittings have been polished, as Lord Curzon says, to a "glassy smoothness" by centuries of exposure to the dust-charged air of Peking.
>
> There is a peculiar interest attaching to these astronomical instruments, as they were designed to replace a set of still older Mongol instruments (dating from 1279), which now repose in the grassy courtyard below. Consequently the appliances of the Jesuit Father may be said to have represented even at that date an Imperial tribute from China to the superiority of the scientific methods of the West. The demolition of the Porcelain Tower at Nanking was bad enough, but that was done by the uncivilized Taiping rebels; the removal of one of the most interesting and venerated antiquities of the northern capital is not creditable to Europeans.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXIV. MARCH, 1901. No. 303.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 February 8.

Mr. E. B. KNOBEL, *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and F. W. DYSON, M.A.

Mr. Dyson read the Minutes of the last Annual Meeting, which were confirmed.

The President. We are assembled here to-day as astronomers, but our zeal for astronomy has been quenched for the time in the more poignant feelings which fill each one of our English hearts. We mourn that gracious Lady, so great, so good, so wide in her sympathies in all that could contribute to the intellectual and social welfare of her people, who was our beloved Sovereign and our august Queen. The expressions of sorrow from every community in the world, especially from those that form British communities, have filled us with pathetic feelings, in that they unite, absolutely unite, in one expression of appreciation of the warmest possible character of that great Sovereign whom we grieve. Your Council who have, as every member of this Society has, received precious favours from our late Queen, in that throughout the whole of her reign she was the august Patron of our Society, have decided to present to H.M. the King an address of condolence with him in his great sorrow, and congratulation that he has succeeded to the throne of his late Mother. I will now read the Address which the Council propose to pass, and which will be presented to His Majesty :—

To THE KING'S MOST EXCELLENT MAJESTY:

MAY IT PLEASE YOUR MAJESTY,—We, the Royal Astronomical Society humbly beg leave to approach Your Majesty with the expression of our deep and respectful sympathy on the death of our late Sovereign, our beloved Queen

Victoria. Her noble life and constant devotion to the welfare of her subjects demands our reverence and has won our deepest affection. We look back with pride to the advancement of science and the progress of the arts which her glorious reign has witnessed, and we rejoice that it was our special privilege that it pleased her to extend to our Society throughout the whole of her reign the same patronage which we received from His Majesty William the IV. While we deplore the loss of a beloved Sovereign, we hail with great satisfaction and congratulation the succession of Your Majesty to the Throne of this great Empire, being fully assured that under the auspices of Your Majesty the welfare and the prosperity of the Empire, and the advancement of science and the arts, will be fully maintained and promoted.

We pray that Your Majesty and your Royal Consort may, under the blessing of God, be granted both health and happiness and a long and prosperous reign.

I ask you to signify your approval of the act of your Council by standing up.

The Address was duly approved, the audience standing.

Mr. Inwards read the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1900, and had found and certified the same to be correct. The cash balance in hand on 1900 December 31 was £549 12*s.* 10*d.*, as compared with £434 6*s.* 2*d.* on 1900 January 1. No changes had been made in the investments of the Society. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in a very satisfactory condition. Mr. Inwards added that the affairs of the Society continued to prosper, and there would soon be occasion to make a further investment.

The Treasurer's accounts and the Auditors' report thereon were adopted by the Meeting.

Mr. Newall read extracts from the Report of the Council for the past year. The Society has lost by death the following Fellows:—J. S. Ancona, Thomas Buckney, Lord Farnham, J. E. Keeler, E. J. Lowe, John McLandsborough, Horace Pearce, W. T. Radford, Sir Josiah Rees, T. G. Rylands, C. Piazzi Smyth, J. H. Young; and Associates J. E. Keeler and Robert Luther.

The Report on the Progress of Astronomy comprised notes on the following subjects:—Minor Planets, Meteors, Figure of the Moon, Prof. Kapteyn's proposal for determining Stellar Parallax, Double Stars, the Greenwich Ten-Year Catalogue (1890), the Cataloguing of Astronomical Literature, the Astrographic Chart, Universal Time, the Cape Photographic Durchmusterung and the Cordoba Durchmusterung, Variable Stars, the Solar Parallax from the French Transit of Venus observations, the Comets of 1900, the Astronomische Gesellschaft Catalogue, Solar Spots and Prominences, Stellar Spectroscopy, and Researches on Stellar Parallax made at the Cape.

During the reading of these extracts the Hon. H. J. Choate, the American Ambassador, entered the room, and was received by the President.

The President. The Council has awarded the Gold Medal of the Society to Professor Edward Charles Pickering, for his researches on Variable Stars and his work on Astronomical Photography.

Mr. Knobel delivered a suitable address on the work of Prof. Pickering, setting forth the grounds on which the award was made; in conclusion he addressed Mr. Choate, and handed the Medal to him for transmission to Prof. Pickering.

The Hon. H. J. Choate. Mr. President and Gentlemen of the Royal Astronomical Society,—It is a source of great regret to Professor Pickering that he is not able to be here in person to-day to receive this signal mark of the approval of this distinguished and venerable Society. But you can well realize that the very heavy responsibilities and duties that rest upon him have made it impossible for him at this season of the year to absent himself from his post for so long as would be necessary to cross the ocean and present himself before you. I esteem it a very great compliment to have been asked by him to come and receive this Medal on his behalf. I regard the conferring of this mark of approval upon so distinguished an American astronomer as a compliment, not only to him, but to the country that is so proud of him. (Loud applause.) I am sure that when you hear the letter of acceptance which I shall have the honour to read, in which he announces for the first time the very latest results of his researches—up to date—you will realize that the bestowal of this Medal has not only already stimulated him, as he says, but will stimulate him in the future to renewed exertions; and not only him, but all the other men of the same science in America, North and South, who look to this venerable Society as the absolute authority in the science which they follow. (Applause.) I will read a copy of the letter, and leave the original for filing on the archives of the Society *. Mr. President, I receive on Professor Pickering's behalf this Medal, and shall transmit to him this signal of your approval with real patriotic national pride. (Loud applause.)

Dr. Johnstone Stoney. It is with pleasure and satisfaction that I rise to propose:—"That the Report now read be received and adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's Address." The Address of the President will add another to the long series of important addresses which stand recorded in our Transactions and Proceedings.

Colonel Burton-Brown. I am very glad to second this motion. I think that we shall be unanimous in adopting the Report which the Council has laid before us, and the statement of accounts. I think the progress that we are making from year to year justifies us in doing so with great pleasure.

---

* Mr. Choate read the letter, in which Prof. Pickering expressed his appreciation of the honour, and in conclusion stated that he had pleasure in announcing for the first time that he had recently obtained photographs, with a telescope of eight inches aperture, of the spectra of stars of the thirteenth magnitude, so faint that the stars themselves were not visible in a telescope of twelve inches aperture. The Medal would, he added, be a great stimulus to widen the broad field that seemed to be still open before him.

The President. We will now take the special business on the Agenda Paper, namely the proposed alteration of Bye-Law 42, which Mr. Newall will introduce.

Mr. Newall. Notice has been given in the letter summoning this Annual Meeting that an alteration of the hour of ordinary Meetings is proposed. It is proposed by the Council that the hour of the ordinary Meetings of the Society be 5 o'clock, and that Bye-Law 42 be altered accordingly. It will be remembered by the Fellows that a similar proposal was brought before the Meeting last year, and that it was then resolved that the matter be deferred for consideration till a later Meeting, or rather, to put it more precisely, the previous question having been moved, we voted it, and so the matter was dealt with. This is a change which is of very considerable importance. There has been a growing feeling, some of us have gathered, that it would be in the interest of the Society that an afternoon hour should be fixed, instead of 8 o'clock in the evening as at present, and it is right, therefore, that this question should be put to the vote, and it is to be hoped that Fellows will vote according to what each one feels to be his own convenience, having due regard also to what he considers to be the real interest of the Society. The value of a change can really only be gathered by an actual experiment, and it should be borne in mind that this change does not commit us permanently if it is adopted on the present occasion. We shall be at liberty at any future time, I take it, to make a second change, but that of course is not contemplated in the present vote. If we decide in favour of a change to an earlier hour of meeting, that decision will be arrived at because the feeling of the majority is that the change is in the interest of the Society *as far as we can judge at present.* Anything that occurs later must be left to the future. I do not think I have anything more to add.

The President. It would be very much more convenient that we should consider this motion as business which we have to consider to-day without any reference to the past. In reference to what took place last year, I do not think Mr. Newall is correct, because the motion then was disposed of and done with. The motion is brought forward again *de novo*, therefore we need not refer to anything that took place last year.

Col. Burton-Brown. Has the motion been duly seconded?

The President. The motion is brought forward by the Council, and it was decided by the Chair last year that a motion brought forward by the Council does not need any seconding.

Col. Burton-Brown. I should like to make a few remarks upon this motion, which has now become an annual one. In a conservative association of this kind we ought to be very careful not to make any change unless we know that the whole body of the

Society approves that change. I rather thought when we had this notice put before us that, perhaps, some ballot-paper, or something of that kind, would be sent round, so that the opinions of Members or Fellows of the Society might be known and laid before this Meeting. Of course, the gentlemen who are present here represent but a very small portion of the Society, and I have heard from a great number of sources that an alteration to 5 o'clock would be in every respect, for a great number of us, exceedingly inconvenient. It might be found more desirable to alter our evening Meetings to an hour earlier than 5 o'clock. In several of our scientific societies the hour is 3 o'clock, or $3^h\ 30^m$, and for many years past the intelligence shown by the Council of the Royal Astronomical Society has been amply borne out by having our Annual Meeting at 3 o'clock. I therefore think that in making any alteration in time we should have the hour fixed for 3 o'clock or half-past, instead of 5 o'clock. There are many reasons why 5 o'clock is an extremely inconvenient hour. It would interfere with our dinner-time, although it may be said that the mental relaxation we receive here is much superior to any physical advantage which we may derive from dinner. I have been told that 3 o'clock might make it rather inconvenient for the Council of the Society, but I think the gentlemen forming the Council, who give up so much of their time and convenience for the benefit of their fellow members, would not hesitate to alter their arrangements for the good of the Society. One thing which ought to be taken into consideration is that many of us are country members, and that by having an evening Meeting we have always the excuse for a day in town. I would therefore propose the following amendment, which I hope some one will second:—"In case of any change being made in our hour of meeting, that 3.30 be fixed as the time of Meeting instead of 5 o'clock, as now recommended by the Council."

Rev. J. T. W. Claridge. I have very great pleasure in seconding the amendment which Colonel Burton-Brown has now brought forward. Speaking as a junior Fellow, as I live at a distance of about 120 miles from town, it would give me very great pleasure to attend at a more convenient hour than the present one of 8 o'clock. It would give me the opportunity of coming up from Staffordshire, leaving by an evening train, and so getting home in good time at night. I appeal for those who live a long distance away, and if the Council can see their way to make any alteration I myself shall be extremely gratified.

Lieut. Dannreuther. I think the Members would like to hear the views of the Council on the proposal that the hour should be $3^h\ 30^m$.

Mr. Alex. Foote. I think the onus of the matter lies with the Council. I think the Council should tell us whether there is any large desire for the change. Personally I do not think 8 o'clock is a good hour for meeting, as it comes so shortly after dinner. Half-past eight would be better. I do not want to turn the Society into a gastronomical Society, but 8 is a bad hour, and so is half-past 3, for then we should not get any afternoon tea. I would suggest $8^h\ 30^m$.

Capt. Noble. I think the announcement in the circular convening the Meeting is, perhaps, a little misleading. "Proposed by the Council." Now any outsider reading that would, I think, suppose that the Council had come to the conclusion, *qua* Council, that it was expedient that the Society should meet at 5 o'clock, and therefore they proposed it. I, as a Member of the Council, distinctly understood it was to bring the matter before the Society for their decision, and that the Council did not, *qua* Council, express any opinion on the matter. I am certain many of the gentlemen in the room think the Council are recommending this. The Council are only putting the question before the Society for its decision. They are not saying one thing or another. I know what I should like: I should like an afternoon meeting very much. It would suit me a great deal better, as I cannot possibly attend an evening meeting without staying in town all night. My last train goes at 7 o'clock from Victoria, and the Meeting at present does not begin till 8 o'clock. It should not go forth that the Council proposes to the Society that it should meet at 5 o'clock. The Council merely say that the question has been mooted, and that it is for the Society to decide the particular hour at which it should meet.

The President. Perhaps I may explain that opinions were expressed to the Council that it was very desirable to have this question ventilated and discussed. The only way that it could be brought forward at this Meeting was by a definite motion. Therefore a motion was made and the resolution was carried by the Council which is now brought forward as special business for to-day. The only possible way of bringing it under consideration and discussion to-day was to do it in the way it has been done. It could not be brought forward in any tentative manner. All our subjects for discussion must be arranged beforehand, and therefore this is the only way in which the subject could come up. I think opinions have been expressed decidedly in favour of the advantage of such a change.

Professor Turner. I should like to say with reference to what has fallen that my opinion as to what happened is certainly not in accordance with what Captain Noble said. The words must be taken as they are printed. I remember the vote taken was one which, to my surprise, showed that a majority of the Council were distinctly in favour of the change. Last year, I may say that had it not been for the rather pronounced opposition of our present President, the change would have been effected. It will be in the recollection of most present that when the proposal was made Mr. Knobel was to be elected President, and it was thought courteous to him not to make the change at the moment he was about to occupy the Chair. I fancy that last year we should have adopted the alterations had it not been for these conditions. I think that on the evidence we have before us that the change to 5 o'clock will be a good one. As has been said by the Secretary, we are quite at liberty to go back to the old hour if we find that the new one proves inconvenient. As to making the hour of Meeting 3.30, I am afraid that would not be convenient for the meetings of Council, which are held previous to the ordinary meetings.

Dr. Common. I wish to move an amendment.

The President. Is this an amendment on the original motion?

Dr. Common. It touches it particularly.

The President. We will dispose of Col. Burton-Brown's amendment, and then we will consider yours. The amendment is that the hour 3.30 be substituted for 5 o'clock.

The amendment was put to the vote and declared lost.

Dr. Common. The Secretary stated that if the time 5 o'clock was found to be unsuitable, the hour of 8 o'clock could be gone back to. I would therefore move that the time of the Meeting be such as the Council may fix from time to time. We would thus leave it in the hands of the Council.

Lieut. Dannreuther. I think that would meet the ideas of everybody present. No one has any strong bias as to the hour of meeting so long as it is in the interests of the Society. I beg to second this amendment.

The Astronomer Royal. This would throw a very invidious task on the Council. That body would be receiving complaints either from those who wish the Meeting to be held in the afternoon, or from those who desire it to be held in the evening. If you hold Meetings alternately in the afternoon and in the evening, there will be constant complaints from both sides.

Col. Burton-Brown. If this amendment is carried, how are Fellows who do not attend one Meeting to know what hour is fixed for the next Meeting? Is it proposed to send out monthly post-cards announcing the hour?

The amendment was then put to the Meeting and received no support.

Rev. S. J. Johnson. It may be possible to find an amendment which, to a certain extent, will meet the views of both sides. I should like to propose that the daylight Meetings in the months of May and June should be held at 8 o'clock as heretofore, and other Meetings at 5 o'clock.

This amendment was not seconded.

Capt. Noble moved that the afternoon Meetings be held at 4 o'clock.

Dr. Kinns. I beg to second that proposition, for then I shall be able to get home by 7 o'clock.

Col. Burton-Brown. I hope the Council will accept that as a fair compromise to the proposal I have made.

The Astronomer Royal. I should like to make a few remarks. I think Fellows must consider how the business of the Society is to be carried on by the Council. It will be extremely difficult for them to meet and dispose of all their business before the Meeting of the Society if it is $3^h\ 30^m$. At the same time the Royal Society have their meetings at 4.30, and the Council succeed in getting through their business before that time, although with some difficulty. If we are to consider the convenience of the Council it will be more desirable to have our Meetings at 5 o'clock. It is, however, practicable to have our Meeting at 4.30, and yet to do the business of the Council before it.

Col. Burton-Brown. I think the Council ought to consider the wishes of members generally, and I say this without any disrespect to the Council.

The amendment was then put to the Meeting and declared lost by a large majority.

A Fellow. It seems to be we are now prepared to vote on the motion as submitted by the Council.

The President. The arguments brought forward relate to the habits and customs with regard to dinner having altered since the Society first started. It is perfectly true that the hour of dinner has become very much later, and this does entail a certain inconvenience to members attending in the evening. It seems a strong argument in favour of a change. Professor Turner has thought proper to remark upon the opposition that I have persistently carried out during the last ten years. It is perfectly true that I opposed this alteration most strenuously throughout this period, and I would still have opposed it if carried. As to the business of the Council, it has been stated that its business can well be conducted in a short time. I take exception to that entirely. There is much business that comes on at the end of the Council Meeting which has to be put on one side because there is not time to consider it. This, however, is a question which concerns the Fellows at large and not the Council. It is a privilege which Fellows have the right to enjoy, and it is for them to decide this question and not the Council at all. Of course, the Council have a voice in the decision. The point of the matter is whether the change will work well—whether it is advisable to make a change. Those are the points which you have to consider and weigh well before you give your vote. If there is no other speaker I will put the motion, that the business of the Ordinary Meetings shall begin at 5 o'clock in the afternoon precisely.

The motion was carried by a large majority.

The Meeting then proceeded to the election of Officers for the ensuing year. Mr. J. J. Atkinson and Mr. Thwaites were appointed Scrutineers of the ballot-papers. The following was the result of the election:—

President: J. W. L. Glaisher. *Vice-Presidents*: W. H. M. Christie (Astronomer Royal), A. A. Common, E. B. Knobel, Prof. H. H. Turner. *Treasurer*: W. H. Maw. *Secretaries*: F. W. Dyson, E. T. Whittaker. *Foreign Secretary*: Sir William Huggins. *Council*: Sir William Abney, Sir R. S. Ball, Prof. G. H. Darwin, Capt. E. H. Hills, Frank McClean, Major P. A. MacMahon, H. F. Newall, Capt. William Noble, A. A. Rambaut, G. M. Seabroke, E. J. Spitta, W. G. Thackeray.

Dr. Glaisher then took the Chair.

It was proposed by *Maj.-Gen. Waterhouse*, and seconded by *Sir Erasmus Ommanney*, "That a vote of thanks be accorded to the President, Vice-Presidents, and other Members of the Council who now retire, for their services in promoting the objects of the Society.".

A vote of thanks was accorded to the Auditors of the Treasurer's accounts and the Scrutineers of the ballot.

The following gentlemen were elected Fellows of the Society:—

Otto Hilger, 204 Stanhope Street, Hampstead Road, N.W.
Rev. Geo. Vickars-Gaskell, Grange-over-Sands, N. Lancashire.

The following Candidates were proposed for election as Fellows of the Society:—

F. A. L. Kitchen, B.A., Naval Instructor, H.M.S. 'Britannia,' Dartmouth (proposed by H. H. Turner).

Rev. Richard Coad Pryor, M.A., The Rectory, Grafton Regis, Stony Stratford (proposed by J. W. L. Glaisher).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The third ordinary Meeting of the eleventh Session of the British Astronomical Association was held on January 30th at Sion College, *Mr. G. M. Seabroke, F.R.A.S.*, President, in the Chair.

The President referred in suitable terms to the death of her late Majesty, and said that it had been suggested that the meeting should be adjourned as a mark of respect, but as many of the members had come from a considerable distance, the Council thought it inadvisable to do so.

Mr. Edwin Holmes read a paper entitled "A Chapter for Astronomical Beginners." The writer remarked that astronomical manuals contained the data of the orbits of the planets and satellites given with the utmost exactness, and descriptive writing of the highest order abounded; the paths were traced on maps with loops, and stationary points and places where they advanced or retrograded on the background of the stars, yet, when the ordinary reader had crammed himself with periods and distances and speculations, he was still at a loss, when confronted with the night sky, and could not bring his book learning to bear upon his personal observations of the celestial motions. Mr. Holmes then proceeded in a very simple and lucid manner to describe the paths of the different planets and to illustrate the reasons for the varying altitudes of the Moon, pointing out, in conclusion, the absurdity of some popular weather proverbs based on the apparent position of the Moon.

Capt. Noble said he must congratulate Mr. Holmes on his style of prophecy, so exactly that of the weather almanacs, which said "It will be wet on Tuesday, unless it is fine, in which case it wont rain." There was a good deal of that sort of thing now, and bringing a little common sense to bear on the matter was likely to prove beneficial in a great many directions. As to what Mr. Holmes had said about the want of knowledge of the face of the sky, there was a curious illustration of that not many years ago on the other side of the Channel, when M. Lescarbault, who would doubtless be recollected as the discoverer of Vulcan, discovered another planet, which was duly announced in the Académie des Sciences; but unfortunately the object turned out to be Saturn.

Mr. Newbegin said Mr. Holmes needed to make no apology in introducing his paper, to which he (the speaker) had listened with the greatest pleasure. There was so much in it that was so thoroughly practical and useful, not only to beginners in astronomy, but even to those who had long been able to realize the true relationships of the paths of the planets and the Sun. He had not the slightest doubt that the paper which had been read would help even members of the Association when they were asked difficult questions by those who were outside; and certainly those outside who were not astronomers, and who had the opportunity of reading the paper, would learn much, and would be greatly assisted in doing so by the extreme lucidity of the paper itself.

Mr. Maunder thought Mr. Holmes had hit upon a real blot on many of their astronomical text-books, viz.: that they did not sufficiently describe and explain the actual appearances presented to them in the sky. He supposed the question most frequently addressed to the Astronomer Royal at Greenwich Observatory, or to himself privately, was to ask, respecting the planets Venus or Mars, "What could that strange star be?" It was clear, from such questions, that there was an almost entire ignorance of the actual aspect presented by the midnight sky night after night amongst the general population of the country, and not merely amongst those who might be called the uneducated. It struck him that if they could bring home to a very much greater proportion of the population than at present took an interest in such matters, that there was a real naked-eye astronomy which did not require instruments, but that simply required them to watch the changing aspect of the heavens intelligently, it would do such people good, and certainly promote a living interest in astronomy.

The President, alluding to the blunders that even educated people sometimes make, said he was reminded of a story that his old friend Archdeacon Wilson used to tell. The Archdeacon, when revisiting his old school in the Isle of Man, was asked by a leading banker of the island why it was that the weather was always fine when the Moon was on her back and able to hold the water; but that when the Moon was tipped up, so that the water ran out, it was always wet weather. He remembered on one occasion showing some University Professors and mathematical masters the planet Venus through the telescope, when one of them remarked, "Why, that is not Venus!—it isn't round!" It was well even for scientists to go back again now and then to the "goose-step."

Mr. Crommelin gave a *résumé* of the report of the Mars Section for 1898–99, submitted by the Director, Mr. E. M. Antoniadi. Both the recent and the coming oppositions were unfavourable as regarded the distance of the planet from the Earth. In the recent opposition its least distance was 60,000,000 miles, as compared with 52,000,000 miles four years ago, and the distance would be even greater at the coming one, which would be the most unfavourable opposition possible as regarded distance. There was, however, one favourable feature in the opposition of two years ago: Mars was near its greatest declination at the time of opposition, so that it was very high up for observers in this country. Moreover, as the report referred to an opposition when the N. side was turned nearer to us than the south, it had the advantage of dealing with a face of the planet which was not so well known. The Section might be congratulated on having amongst its members such skilful observers as Capt. Molesworth and the Rev. T. E. R. Phillips. The recent areographic work of the former, based on 172 drawings of the planet, was a worthy sequel to that made at the 1896–97 apparition. Mr. Phillips was even more successful in 1898–99 than two years previously. His results had been so fully confirmed from all quarters as to stamp him as one of the very first observers in the British Isles. By his detection of the canals as edges to diffused shadings, the Rev. P. H. Hempthorne had done more than anybody else in these days to increase our knowledge of these enigmatical lines. Notwithstanding the natural scepticism of many scientific men, every opposition brought with it its own contingent of confirmation of Schiaparelli's discovery of linear markings apparently furrowing the surface of the planet Mars. At the last apparition the canals had been seen by all the members of the Section. The report contained several illustrations, and some of these were thrown on the screen with the aid of the lantern.

Mr. Maunder did not think it possible to discuss adequately Mr. Antoniadi's most admirable report from simply hearing it; but there was one fact which had come strongly home to his mind, and that was the immense amount of labour and skill which that gentleman had bestowed upon the discussion of the material with which the Section had supplied him. He, the speaker, had a rather painful recollection of the amount of work which the 1892 report gave him, and Mr. Antoniadi had had a very much heavier task before him in connection with the report now presented, and he was certainly to be very heartily congratulated upon the success which had attended his labours.

Mr. Newbegin was inclined to think the report just presented was one of the most valuable that had ever been submitted by the Mars Section. There was positive confirmation by many observers of the markings known as canals, and they also for the first time learned that these canals crossed not only the bright regions of the planet but the dark regions also. That would help in the future towards showing what these canals really were.

Capt. Noble said the drawing of the late Mr. Green would never, in his (the speaker's) humble opinion, be surpassed as accurate representations of the surface of Mars as it exists. Mr. Green held the opinion, which Mr. Antoniadi attributed to Mr. Maunder, viz.: that these so-called canals (he said "so-called," because he considered the word "canals" a perfect solecism, for in any terrestrial sense it was physically impossible that they could be canals) were the edges of shadings. Among the drawings of these so-called canals just shown on the screen there was one with a straight line extending across the planet, as if it had been ruled. Now what sort of a marking would appear as a straight line for three or four thousand miles across a spherical planet?

The President said that, whatever might be their opinions on the canals of Mars, he was sure they would agree that Mr. Antoniadi had shown great energy, and had taken an enormous amount of trouble, in preparing his report and producing the maps which accompanied it.

Mr. H. Keatley Moore, *Col. Burton-Brown*, and *Mr. Maunder* described the best ways of reaching Sumatra for the benefit of any members who might wish to observe the forthcoming eclipse in May, giving dates, and fares and weather prospects, &c. The meeting was then adjourned to February 27th.

ROYAL METEOROLOGICAL SOCIETY.

The monthly Meeting of this Society was held on Wednesday evening, February 20, at the Institution of Civil Engineers, Westminster, *Mr. W. H. Dines*, President, in the Chair.

A loyal and dutiful Address of condolence and homage to His Majesty the King was agreed to.

Mr. E. Mawley presented his Report on the Phenological Observations for 1900. During the greater part of the winter and spring the weather proved cold and sunless, but in the summer and autumn the temperature was, as a rule, high, and there was an unusually good record of bright sunshine. As affecting vegetation the two most noteworthy features of the phenological year ending November 1900 were the cold, dry, and gloomy character of the spring months and the great heat and drought in July. Throughout the whole of the flowering-season, wild plants came into blossom much behind their average dates, indeed later than in any year since 1891. Such spring emigrants as the swallow, cuckoo, and nightingale were also later than usual in visiting these shores. Taking the British Isles as a whole, the crops of wheat, barley, and oats were all more or less under average. The yield of hay was poor in the southern half of England, but elsewhere varied from a fair to an abundant crop. Turnips and swedes were almost everywhere deficient, but there was a heavy crop of mangolds. Potatoes were under average. This was a bountiful year as regards fruit, the yield of apples, plums, and all the small fruits being in excess of the average.

Mr. A. E. Watson read a paper entitled "A Review of past Severe Winters in England, with Deductions therefrom." From an examination of the records of the severe winters of the last 300 years, he has come to the conclusion that they are most frequent in the years with the numbers 0–1 and 4–5. He is also of opinion that the severe winter in the middle of each decade is generally a late one (January to March), while that at the beginning or end of each decade is generally an early one (November to January).

Modern Astronomy *.

"OMNIA mutantur," wrote Borbonius, "nos et mutamur in illis"; nor would it be easy to find a more striking illustration of the truth of this trite aphorism than that afforded by the marvellous change, alike in instruments, methods, and results, which has characterized the astronomy of the concluding quarter of the XIXth Century, and which is well described as a "revolution" by the author of the really admirable little book, as instructive and interesting as it is merely readable, now lying before us.

There can be no doubt, as pointed out in his preliminary remarks by the Savilian Professor, that, prior to about the year 1875, astronomers as a body felt with a kind of smug self-satisfaction that something approaching to finality had been reached in the methods of astronomical work, and that anything that was to be effected or discovered must be so discovered in modes which had, as it were, become crystallized, not to say perfected, and, as it were, sanctified by long use and experience. Let us, then, glance at the condition of English astronomy, and incidentally at the astronomy of other nations, prior to the year 1875, the epoch selected by our author as that at which modern astronomy took its rise. When Airy succeeded Pond as Astronomer Royal, his first care was to furnish the Royal Observatory with instruments of the highest class. The old mural circle and the transit instrument were combined in the form of that transit-circle which is even now unsurpassed for meridian work. He devised an altazimuth for the observation of the Moon out of the meridian, and invented a form of the reflex zenith-tube (the idea of which, by the way, dates back to Flamsteed) that by the observation of γ Draconis, a star crossing the zenith of Greenwich, the corrections in the star's position due to aberration, parallax, &c. might be determined with all needful accuracy. And last, and, nowadays we should say, by no means least, he set up what was long known as the south-east equatorial, carrying a refracting telescope of 13 inches aperture, of which last addition, though, he wrote in his report for 1859:—"I can only trust that while the use of the equatorial within legitimate limits may enlarge the utility and reputation of the Observatory, it may never be permitted to interfere with that which has always been the staple and standard work here." There is surely in this utterance something reminiscent of the unfortunate Irish lady who, driven by dire distress to cry goods for sale in the street, said that she did so in so low a tone of voice that she hoped nobody heard her! The fact is, or was, though, as the first "King's Astronomer," Flamsteed was commanded by Charles II. "to apply himself with the most exact care and diligence to the Rectifying the Tables of the Motion of the Heavens and the Places of the Fixed Stars, in order to find out the so much desired Longitude at Sea, for the perfecting the Art of Navigation"; so anything outside of this immediate object was regarded by Airy as an unwarrantable interference with the prime and capital end of the Royal Observatory, and as a distraction of the observers from their legitimate duty. It was all very well for the amateur to employ the equatorial for the scrutiny of the physical detail of the Moon and planets, for the measurement of double stars, and the like; but that such scientific frivolity should intrude into the curriculum at Greenwich was a thing, if not to be prevented, at all events to be apologized for. Of the magnificent work done with the transit-circle it is superfluous to remind anyone who will read these lines. The form of altazimuth devised by Airy was not such a conspicuous success; it did, to a certain extent, help in the construction of lunar tables, but, on the whole, it by no means fulfilled its original promise of usefulness. It has been superseded, as we shall immediately see, by a transit-circle which may be laid permanently in any azimuth. Nor was the reflex zenith-tube much more successful. We imagine that its use ceased with Airy's retirement. Such, then, briefly, were the instruments in use at Greenwich up to the date assigned by Professor Turner as that of the dawn of modern astronomy; and although work of the most valuable kind was performed with them, it was essentially meridian work, and hence of the most circumscribed character. We have said above that, *inter alia*, the measurement of double stars was, to a very great extent, in the hands of amateurs; and as late as 1873 we find so distinguished a member of that body as the late Prebendary Webb writing to him who is now *facile princeps* in the field: "It will hardly be possible for you to go on for any great length of time as you have begun, because the number of such objects is not interminable"; albeit since then Professor Burnham has added more than 1000 new pairs to his own list! Verily the earnest astronomer must eschew Lord Russell's injunction to "rest and be thankful."

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* **Modern Astronomy. Being some Account of the Revolution of the last Quarter of a Century. By Herbert Hall Turner, F.R.S., Savilian Professor of Astronomy and Fellow of New College, Oxford. Westminster: Archibald Constable & Co., Ltd., 1901. 6s. net.**

Mr. Christie assuredly did, for on his advent to the position of Astronomer Royal he at once set to work to bring the Royal Observatory into such a state of efficiency, alike as regarded its instruments and the modes of employing them, as should enable it to maintain its position as the head of all such institutions, and to enable it to cope with the problems which were then beginning to present themselves for solution. The 13-inch equatorial patronized by his predecessor was replaced by one of 28 inches aperture, and the old altazimuth superseded by what is, as we have previously said, really a transit-circle capable of being fixed rigidly in any given azimuth. But the 28-inch telescope is not the only large instrument that has been added to the equipment at Greenwich. Sir Henry Thompson has presented a great photographic refractor of 26 inches aperture, with a 30-inch reflector, made by Dr. Common, attached to the same mounting. Another large instrument for solar photography has also been presented to the Observatory by the same munificent donor. We have dwelt somewhat at length on the changes which have been made at Greenwich in its instrumental equipment, because they will serve more or less to explain the alteration in methods which has taken place during the period under review.

Now our reference to the photographic telescopes in use at the Royal Observatory may well serve as an introduction to one of the two chief factors which have gone to revolutionize astronomy during the last quarter of a century. We refer, of course to celestial photography and spectroscopy. When the late Dr. De la Rue, in 1857, initiated the taking of solar photographs at Kew, very little did he, or could he, think to what vast dimensions the photography of the heavens would grow, or to what results of the most vital and capital importance it was destined to lead. We are reiterating a very familiar fact indeed in saying that daily photographs of the solar surface are now taken both in the Old and New World, in both the eastern and western hemispheres. But the Sun is very far from being the only object in the sky to which the telescope of the photographer is systematically directed. At the Paris Observatory, and again at Prague and at Mount Hamilton, pictures of the Moon of unsurpassable beauty—and, *ex necessitate*, accuracy—are in the course of production. Professor Barnard has applied this means to the delineation of comets with conspicuous success, while Dr. Isaac Roberts's invaluable portraits of nebulæ, clusters, and other details of the stellar universe are familiar to all, it being difficult to exaggerate their importance as supplying means of future reference and comparison. Undoubtedly, however, *the* application of photography to the study of the celestial vault of the highest interest and most far-reaching effect which has distinguished the concluding quarter of the XIXth Century is that of the charting of the entire heavens, which has been the outcome of the International Astrographic Congress which met in Paris in 1887. The advantage of this method of mapping the stars, visible and invisible, it would be impossible to overestimate; notably since it has been found that the micrometrical measurement of the positions of the stellar images on the plates by means of rectangular co-ordinates is susceptible of a degree of accuracy perhaps even surpassing that of the results obtained by the transit-circle. Among those who have contributed to this capital effect, the Savilian Professor himself may legitimately say with Æneas "et quorum pars magna fui." Micrometrical measures of lunar photographs promise to supply a means of minute accuracy in charting the Moon which have hitherto been wanting. Photography, again, has, by the ingenuity of Captain Hills, been applied to the curiously unlikely object of fixing the Moon's place among the stars at any given instant, and so determining terrestrial longitude. Our author's faith in its future development is seemingly unlimited: "sooner or later," he says, "the photographic plate is bound to replace the observer at the eye-end of the transit-circle, though as yet there is little progress in this direction to report." Among other advantages, this must eliminate that puzzling defect familiar as "personal equation."

Of celestial spectroscopy waning space warns us that we must treat but briefly. It would boot nothing to retell here how this was practically initiated by him who is *facile princeps* among British observers with the spectroscope, Sir William Huggins, nor with what success it has been employed, by himself, McClean, Newall, Evershed, Lockyer, and others in this country, and at Harvard, Potsdam, and elsewhere abroad. Already has it given us information of the highest value as to the chemical constitution of the Sun, the planets, and the fixed stars, and enabled something like a classification of the latter to be made on scientific grounds. Moreover, by the application of what is known as "Döppler's principle" the rate of motion of various celestial bodies in the line of sight has been measured, the constitution of the remarkable system of the variable star Algol established, and the rotation of Saturn's rings made, as it were, visible. These are but a few of the typical results obtained. We must hasten to briefly review some modern astronomical results.

Among the most curious and, save to the gravitational astronomer, least explicable of these is the establishment by Mr. Chandler of the rotation of the pole of the Earth's axis in a period of some 14 months. Its path is one of very remarkable intricacy. Were it a circle it would have a radius of less than 36 feet; and yet, owing to the refined nature of modern observation, this tiny orbit creates a measurable periodic change in terrestrial latitudes. Another result has, thanks to Sir David Gill and the heliometer, been achieved in the much more accurate determination of the solar parallax, the transits of Venus, so largely depended upon formerly for finding this constant, having been seriously discredited by the glaring discrepancies revealed when the observations made in 1874 and 1882 came to be reduced. The comparatively recent discovery of the minor planet Eros, which, under the most favourable circumstances when in perigee, is situated at a distance of only 0·15 of the Earth's mean distance from the Sun, promises to supply a means of obtaining this constant with great accuracy. It does not seem very likely, however, that the present adopted quantity of 8″·8 will be materially altered one way or another. Among the discoveries of the last 25 years may be mentioned those of two satellites to Mars by Professor Asaph Hall, of a fifth satellite to Jupiter by Professor Barnard at Mount Hamilton, and (albeit this is dubious) of a ninth satellite to Saturn by Professor W. H. Pickering.

It only remains to refer briefly to the advances in mathematical astronomy which have characterized the latter portion of the Victorian reign. It was certainly believed, as said Dr. Glaisher in 1887, "that the mathematical portion of the treatment of the lunar theory had been worked out." Most of our readers will remember the numerical theory with which Airy occupied his declining days, and its sad collapse. About the date specified, however, Dr. G. W. Hill, of the American Nautical Almanac department at Washington, startled mathematicians by approaching this somewhat abstruse subject from an entirely fresh point of view, resulting not merely in a simplification of it, but in the attainment of increased accuracy; while a perhaps even more astonishing advance has been made in this country by Professor G. H. Darwin by his researches into the genesis of the Earth and Moon, and the cause and effect of their dissociation on their changing distance and varying periods of rotation and revolution.

But we must conclude, after having, as it were, merely touched the fringe of the subject. The imperfect *prècis* which we have been able to give, within our prescribed limits, of Professor Turner's luminous and instructive work will, we can only trust, send the reader to that work itself to perfect his information. Assuredly from no other extant will he be able to derive so clear and intelligent an idea of the capital advances made in modern astronomy.

William Noble.

Temporary Stars.

As though the beginning of the new century had not been sufficiently marked by sensation, astronomy has furnished another by the appearance of a star of the first magnitude in the constellation Perseus. The discovery will doubtless be credited to the Rev. Dr. Anderson, of Edinburgh, a retired Congregational Minister, who discovered Nova Aurigæ in 1892, but although he was the first to send news of the discovery to the Astronomer Royal, there are several others who saw and recognized the object as a stranger without previous knowledge of its advent; in fact, given a clear sky to anyone conversant with the heavens, the discovery could scarcely have been missed.

Such objects are rare. There are only about a score on record, as will be seen from the following list, taken from Miss Clerke's 'System of the Stars.' The date of discovery and the constellation in which the object appeared are here given :—

134 B.C.	Scorpio.
123 A.D.	Ophiuchus.
173	Centaurus.
386	Sagittarius.
389	Aquila.
393	Scorpio.
827	Scorpio.
1012	Aries.
1203	Scorpio.
1230	Ophiuchus.
1572	Cassiopeia.
1604	Ophiuchus.
1670	Vulpecula.
1848	Ophiuchus.
1860	Scorpio.
1866	Corona.
1876	Cygnus.
1885	Andromeda.

To these must be added the Nova in Auriga of 1892 and two discovered in Norma and Carina in 1893 and 1895 respectively, on photographic plates taken at Harvard. It will be seen that such discoveries have been much more frequent during the last half-century than previously, which seems natural, as in earlier years doubtless only the brightest Novæ were observed and recorded. To find a parallel to the present case it is necessary to go back nearly three centuries, to the year 1604, when the star known as Kepler's, but discovered by Brunowski, appeared as bright as Jupiter. Tycho's star, in 1572, was first seen as bright as Jupiter and increased quickly, so that it became equal to Venus; the star in 1203 is said to have been equal to Saturn, and others early in the list were equally conspicuous. Of the later instances, T Coronæ, discovered by Mr. Birmingham, was the brightest. This suddenly appeared of the 2nd magnitude, although the constellation had been observed only a few hours earlier by Schmidt at Athens, who was confident that the star could not then have been brighter than the fifth. Nova Cygni, in 1876, was of the 3rd magnitude when discovered, but rose to the second. T Coronæ is also remarkable because this and the star seen in 1012 are the only two of the list which lay far from the Milky Way. This fact is not without significance, and in this connection it is worthy of note that the star of 1860 appeared suddenly as a 7th magnitude star in the middle of a nebula or close cluster (Dreyer 6093), and that the star in Andromeda was seen by Dr. Hartwig on August 31, 1885, as a nucleus to the well-known nebula.

The more recent examples of Novæ have naturally been submitted to spectroscopic examination. T Coronæ was carefully studied by Sir William Huggins, who pronounced its spectrum to be due to glowing hydrogen, as though it were a Sun like our own with very exaggerated prominences; the star Nova Cygni, as seen by Vogel, had at first a spectrum nearly continuous, crossed by bright lines due to hydrogen, and by others due to unknown substances, but finally became similar to the spectrum of a nebula. The star of 1885 showed a continuous spectrum with bright lines very feeble; but the Nova in Auriga, when submitted to the spectroscope, gave rise to new surmises. The spectrum was not only crossed by bright lines due to hydrogen and other elements, but also by dark lines due to the same substances, the bright and the dark lines being displaced relatively to each other. This peculiarity was interpreted by Prof. Lockyer and others as an evidence of relative motion of two bodies; and from this it was assumed that Nova Aurigæ was actually two bodies which had collided, and so an outburst of heat was caused from loss of kinetic energy. It seems that Sir Norman holds this view in the present case of Nova Persei *. The hypothesis, at first sight, looks enticing; but the probability of the meeting of two runaway stars seems small, and especially as the spectrum for one star must be, as it were, the shadow of the spectrum of the other. It has been pointed † out that the observation could be equally well interpreted as the spectrum of a body of the solar type from which hydrogen gas was emanating with the velocity shown by the displacement of the lines, which is not an inconceivable one. The Novæ in 1893 and 1895 both showed spectra with the same peculiarities.

As to the "hereafter" of these remarkable objects, Tycho's star remains in the heavens, a shadow of its former greatness, as the variable β Cassiopeiæ; Kepler's has gone and left no trace. There is reason for thinking that the Nova Cygni of 1670, which lasted as a fairly bright star until 1672, was identical with an 11th magnitude star picked by Mr. Hind in 1852. T Coronæ, in 1878, had resumed the condition that it had prior to 1866. The Nova Cygni of 1876 was seen as a 15th magnitude star in 1885, and probably so remains. There is now no trace of the suddenly arrived nucleus in the nebula of Andromeda nor in Dreyer 6093, and Novæ Aurigæ has not been seen for many years. Such are some of the facts respecting these wonderful bodies, and it is difficult to see how it is possible to decide between the conflicting interpretation of the observed spectra, but it does not require a vivid imagination to realize that we are witnessing a stupendous natural phenomenon. Possibly the injunction, "Let there be light, and there was light," of Genesis is being obeyed many million miles away.

H. P. H.

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* See p. 138.

† See *Observatory*, vol. xv. pp. 334 & 361.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Ancient Eclipses of the Sun.

GENTLEMEN,—

Prof. Newcomb has some remarks, in No. 3682 of the *Astronomische Nachrichten*, on ancient solar eclipses, with special reference to Prof. Ginzel's great work on the subject under the title 'Spezieller Kanon der Sonnen- und Mondfinsternisse.'

Prof. Newcomb adds his testimony to the high appreciation by astronomers and chronologists of the great historic value of the exhaustive researches set forth in this very interesting volume; but expresses his doubts as to whether it is possible to base upon them corrections to the elements of the Moon's orbit. On this point it would ill become me to offer any opinion; but having written in the *Observatory* several times on various ancient and medieval eclipses, I beg to be allowed once more space for a few words with regard to two of the former, in which Prof. Newcomb considers that the historical accounts given are as of total eclipses, though the eclipses which occurred were not really total at the places of observation.

I do not intend to refer again to what is commonly called the eclipse of Xerxes, mentioned by Herodotus. It seems to me to be one of the insoluble points in history, and I have nothing to add to the suggestions which are contained in my 'Remarkable Eclipses.' The two eclipses to which I wish now to refer are those of B.C. 763 and of B.C. 431. I must premise that Prof. Newcomb speaks of them as of −763 and −431; the real dates are B.C. 763 (= −762), June 15, and B.C. 431 (= −430), August 3. Of the former Prof. Newcomb says, "which we might suppose to have been total at Nineveh . . . . . the southern edge of the shadow passed about 100 miles north of Nineveh." Now the Assyrian tablet does not state that the eclipse was total; no doubt it was very large at Nineveh and all over Assyria. Of the solar eclipse in B.C. 431 (the first year of the Peloponnesian war) Prof. Newcomb says, "which Thucydides describes as total . . . . there is only an annular eclipse, the path of which did not come within 500 miles of Greece." Now, as a matter of fact, Thucydides expressly states that the eclipse in question (it was on this occasion that he appears to think he had made a remarkable discovery, that an eclipse of the Sun occurs only at the time of New Moon) was *not* total. His words are (ii. 28):—"*ὁ ἥλιος ἐξέλιπε μετὰ μεσημβρίαν καὶ πάλιν ἀνεπληρώθη, γενόμενος μηνοειδὴς*": *i. e.*, "the Sun became crescent-shaped and then recovered its fulness." He goes on to say that some stars appeared, which would certainly imply, if taken literally, at least a very large partial eclipse. But it must be remembered that we are not quite certain where the historian was at the time. Thucydides had estates in

Thrace, and was frequently in that region, from which he would have reports when absent. The central line would be much nearer there than at Athens, since it passed over the north-western shore of the Euxine in a south-easterly direction. With regard to the fright of the pilot of Pericles mentioned by Plutarch, any visible alteration in the apparent shape of the Sun would be quite enough to cause that, and the cloak would be equally likely to relieve it whatever was the amount of obscuration.

Yours faithfully,

Blackheath, 1901, Feb. 13. W. T. Lynn.

OBSERVATORIES.

U.S. Naval Observatory, Washington.—For years past there has been amongst American astronomers a growing irritation against their National Observatory at Washington. They consider that the work turned out by the Institution has not been commensurable with the outlay, that, indeed, the chief object of a National Observatory—continuity of fundamental routine work—has been neglected. The Head of the Observatory is a naval officer, and the astronomers contend that it is a mistake to suppose he can prove as efficient a Director as an experienced astronomer.

In 1899 the Secretary of the Navy appointed a Board of Visitors, consisting of Hon. W. E. Chandler, Prof. G. E. Hale, Hon. A. G. Dayton, Prof. G. C. Comstock, and Prof. E. C. Pickering, and authorized them to examine the equipment and work of the Observatory, and to make any suggestion for improving its scope and usefulness. Briefly their report was as follows:—

1. The control of the Observatory by a naval officer seems illogical and undesirable.
2. If the Observatory must be considered as a shore station, to do the Navy work under the command of a naval officer, then the astronomical staff should be independent and consist of an Astronomical Director at $6000, First Astronomer $4000, Second Astronomer $3600, Third Astronomer $3200, Fourth Astronomer $2800, three Assistants from $2000 to $2400.
3. That the Nautical Almanac should form a distinct bureau with a Director at $5000.
4. In deference to the long-continued criticism against the administration of the Observatory, a Board of Visitors be appointed, consisting of six astronomers of high standing and three eminent citizens.

Other suggestions and criticisms which the Visitors have made will appear from an analysis of the Superintendent's Report, which we have now before us. This report is for the fiscal year ending 1900 June 30, and in it Captain Davis at once makes clear his hostility to the compromise of the B. O. V. He states that the Board made no examination of the Observatory except in the most casual way, and, to quote its own language, "Owing to the lack of printed material representing the recent work of the Naval Observatory, the Board of Visitors finds it practically impossible to form a satisfactory opinion of that work without devoting to the task an inadmissible amount of time and labour." Nevertheless, says the Superintendent, the Board did not hesitate to recommend the most sweeping changes in the present organization. It would appear the Board based this recommendation on a comparison between the Naval Observatory on the one hand and the observatories at Greenwich and Harvard on the other; the Board selecting the former because it is supposed to cover the same field of work, and the latter because it also undertakes a large amount routine work. The Superintendent therefore endeavours to confute their conclusions by a comparison of the cost of each, and we give here the tables as being of general interest, beyond that of the present case:—

Greenwich.

	Number.	Average.	Total.
Astronomer Royal	1	$5,000	$5,000
Chief Assistants	2	2,550	5,100
First Class Assistants	4	2,000	8,000
Second Class Assistants	2	1,200	2,400
Established Computers	4	550	2,200
Computers	19	325	6,175
Magnetic Superintendent	1	2,000	2,000
Established Computer	1	550	550
Computers	4	325	1,300
Total	38	......	32,725

Harvard.

Director	1	5,000	5,000
Astronomers	6	2,000	12,000
Assistant Astronomers	13	900	11,700
Computers	18	600	10,800
Total	38	......	39,500

Naval Observatory.

Astronomical Director	1	3,500	3,500
Other Professors of Mathematics	4	2,700	10,800
Assistant Astronomers	3	1,900	5,600
Computers	6	1,200	7,200
Photographer	1	1,200	1,200
Total	15	......	28,300

The remarks of the Superintendent, whose salary does not appear in the above Table, on the above comparisons are perhaps a little vague, and it is difficult to see how they help his case. He says that the number of

	at Greenwich.	at Harvard.	at Washington.
Astronomers	3	7	5
Assistants	8	13	3
Computers	23	18	6

and consequently the Naval Observatory has a disadvantage in the number of Assistants and Computers.

Now it seems to us that he would have considerably strengthened his case by saying he had *no* computers (as the term is understood at Greenwich and Harvard), but that all his work had to be done by those who ought to have been employed in a higher grade.

Leaving out the Computers, the really professional staff, if we include the Directors, is as follows:—

Greenwich......	15 Astronomers	$25,250
Harvard........	20 ,,	28,700
Washington	15 ,,	28,300

The delay in issuing the volumes, Captain Davis maintains, is due partly to disorganization consequent on removal and partly to insufficient help. This latter reason is evident from the above figures. For an additional $3000 five computers could have been employed to reduce the work and read proof-sheets. As the Captain says, if once the printing gets behind it is a hopeless task to pull it up with just the same staff as allowed to keep current work going.

The other parts of the Board's Report border on the personal, or have merely local interest. We hope a satisfactory way out of the difficulty can be found, as the Observatory must suffer by its unsettled state.

Turning now to the Superintendent's Report proper, we see that, however uncompromising his attitude towards the B. O. V., "on January 1900 a more efficient distribution of the personnel engaged in the routine work with the transit-circle was undertaken." The nine-inch transit was under repair for six months of the year. The work done with the two transits is summarized by saying that with the 6-inch 1576 observations of stars were made, 323 of planets; with the 9-inch 998 of stars, 208 of planets—making in all 3105 meridian observations. The reductions are complete to March 1900, the copy for press is complete to June 1898, and the copy for 1899 will be in the hands of the printer before 1900 October.

The volumes for 1891 and 1892 are still in the hands of the Government printer, awaiting some changes in, or additions to, the Appendix for 1891, containing "Observations of the Transit of Venus, 1874."

The future publications of the Observatory, beginning with 1893, will be issued as follows:—

Vol. I. "Observations of the Sun, Moon, and Planets, 1894–1899."

Vols. II., III. "Southern Zone, $-13^\circ\ 50'$ to $-18^\circ\ 10'$."

Vol. IV. "Zone Catalogue, $-13^\circ\ 50'$ to $-18\ 10'$."

The most satisfactory feature of the Report is the apparent determination to make full use of the 26-inch refractor. In December 1899 Dr. See was given charge of this fine telescope, and he has lost no time in bringing it back to its proper position amongst large instruments. His observations and discussion of the dimensions of the planets, and his work on difficult double stars we have occasion to notice in each of our issues. These are published already in the *Astronomische Nachrichten.*

NATAL.—The delay in receiving the Report from this Observatory for 1899 seems to have been due (we thought, perhaps, the war was the cause) to a fire at the printer's, which destroyed Mr. Nevill's original Report, so that a duplicate had to be prepared. The obvious effect of the war is seen in the monthly meteorological returns from the outlying stations, for the columns for October, November, December, under headings Dundee, Ladysmith, Upper Tugela, Newcastle, and Umsunga are blank, besides which Mr. Nevill states that during the war the instruments at a number of the meteorological stations have been destroyed and will have to be replaced. The Director has got a house built for himself, partly at his own expense, as the sum granted was found insufficient; also because the vote for the purpose was insufficient, the Assistants' salaries for May and June remain unpaid by the Government. As to the astronomical work of the Observatory, we quote the Report almost *in extenso*:—

> Owing to the reduction made on the annual vote for the Observatory during the last two years, it has been found necessary to suspend the greater part of the scientific work which was being carried on, and restrict the work done to the ordinary routine observations necessary for the determination of time, &c. Some Cometary and minor planet observations have been obtained, and in compliance with the request of the Imperial Observatory at Pulkowa, observations were made of the Lunar Eclipse, when a fair number of occultations were observed. New Lunar Tables have been calculated, and were about to be forwarded to England for printing and publication, when they were to a great extent destroyed by the flooding of a portion of the Observatory during a heavy rainstorm. They will have to be re-written to render them legible, the ink having run.

PUBLICATIONS.

ETUDES SUR LA STRUCTURE DE L'UNIVERS, PAR W. STRATONOFF. —Many astronomers from the days of Herschel, and, for anything we know, many astronomers before them, have considered the attractive subject of the law of the distribution of the stars. In recent times Argelander, Gould, Seeliger, Schiaparelli, and Kapteyn are the names most prominently associated with the question. The 'Cape Photographic Durchmusterung' and the 'Cordoba Visual Durchmusterung' have in the last few years added largely to the available data, and the International Photographic Chart of the Heavens will add still more largely, so that we may confidently predict large accessions to our knowledge in this field of astronomy during the next quarter of a century.

M. Stratonoff's paper, which is published by the Observatory of Tachkent, is based on the 'Bonn Durchmusterung.' He divides the sky from the pole to −20° Dec. (the limit of the B. D.) into areas of approximately 5° square by parallels of declination 5° apart, and meridians suitable at different declinations, and counts the number of stars in each of the 1800 areas given in the B. D. as of magnitudes 1·0–6·0, 6·1–6·5, 6·6–7·0, 7·1–7·5, 7·6–8·0, 8·1–8·5, 8·6–9·0, 9·1–9·5. The mean density of the sky in each of these classes of stars he denotes by 10, and on this scale determines the density of each area into which the sky has been divided. Thus in the first class the density varies from 3 to 22. For each class M. Stratonoff draws a map giving the density in each area and draws the contour-lines and shades the maps so as to exhibit very clearly to the eye where the density is 10, 15, or 20. The charts are discussed separately with respect to the distribution of stars in relation to the Milky Way. They show that the lines of maximum density do not lie on the Milky Way, and that the pole of the Milky Way is not the place of minimum density, and that the point of bifurcation of the Milky Way between Cygnus and Sagittarius has no counterpart in the distribution of the stars as far as $9^{m}\cdot5$. The Milky Way is best regarded as "an agglomeration of great condensations or stellar clouds touching one another all along the galaxy." M. Stratonoff also considers and gives maps of the distribution of stars according to their spectral classes, also of nebulæ and of stellar clusters. F. W. D.

LA CIRCULATION GÉNÉRALE DE L'ATMOSPHÈRE *.—In this admirable work M. Brillouin aims at presenting to the French reader, in a compendious shape, selections from some of the original memoirs of English, American, and German philosophers dealing with the circulation of the atmosphere. In an able prefatory note M. Brillouin briefly introduces the subject, and then presents, in succession, extracts from the works of Halley and Hadley, two of the earliest English writers on the subject; following with a

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* 'Mémoires originaux sur la Circulation générale de l'Atmosphère: Halley, Hadley, Maury, Ferrel, W. Siemens, Möller, Oberbeck, von Helmholtz. Annotes et commentes par Marcel Brillouin.' Paris, 1900.

brief notice of Maury's theories, and copious extracts from the important investigations of Ferrel; and finishing with the later theories of the German philosophers Siemens, Möller, Oberbeck, and von Helmholtz. The printing of the book is of the usual excellence which characterizes French works, and there are very few typographical errors. W. C. N.

NOTES.

Comet Notes.—*Ast. Nach.* 3684–85 contains an investigation of the orbit of Comet 1898 VII. (Coddington-Pauly) by Mr. C. J. Merfield, of Sydney. The comet was discovered by photography at the Lick Observatory on 1898 June 9. Mr. Coddington took a photograph of the nebulous region north of Antares, on which the comet appeared as a strong trail. The comet ran south, and was before long out of reach of northern observers. But the observers at the Cape and Cordoba, and the indefatigable Mr. Tebbutt, of Windsor, N.S.W., followed it assiduously up till the middle of February, 1899. After six months it was again picked up at the Lick Observatory, and five observations were obtained, the last being on 1899 Dec. 6. The total number of observations exceeds 400, but a few have been bracketed, owing either to large residuals or unsatisfactory places of the comparison stars. The number of the latter is nearly 300, and their places are taken from the usual catalogues, weighted according to their estimated reliability, without applying any systematic corrections to the catalogues. A few places are taken from the Cape and Cordoba Durchmusterungs, but these have not been used in the final investigation. Mr. Merfield used as a first approximation provisional elements of his own construction, and computed an ephemeris which was compared with the observations. It was found that the residuals in each element could be expressed in the form $A+Bt+Ct^2$. The observations were weighted in proportion to the smallness of the difference between the actual residual and that given by the formula, but it does not appear that any investigation was made of possible systematic differences in the residuals given by different observers, which makes this method of weighting the observations open to criticism. However, the weights assigned have very little influence on the final result, excepting the last group of observations, viz., those made by Mr. Coddington in the autumn of 1899, which rightly receive a high weight, both from their relative accordance and the large arc of the orbit which they represent.

Sixteen normal places were then formed, the first eleven being in 1898, the others in 1899. Perturbations by Venus, the Earth, Mars, Jupiter, and Saturn were computed at 20-day intervals and applied to the residuals of the normal places, so as to determine the undisturbed orbit.

The equations of condition to find the corrections to the assumed elements were formed and solved by least squares in the usual manner, the following being the final elements:—

Epoch of osculation 1898 June 21.
T .... 1898 Sept. 14·044206 G.M.T.
ω .... $233^\circ\ 15'\ 18''{\cdot}7$
☊ .... $74\ \ 0\ \ 58{\cdot}2$ } 1900·0.
i .... $69\ 56\ \ 0{\cdot}4$
log q...... 0·230859
log e...... 0·000449
e .... 1·001034

The residuals between the normal places and those calculated from the above elements are given. There is a marked preponderance of the positive sign in the residuals in Right Ascension, which seems to imply that the elements need a little modification, but the amount would be very small.

Mr. Merfield is to be congratulated on having brought this laborious piece of work to a successful conclusion. He has from the first interested himself in this comet, and published successive emendations of its orbit elements. The final result, to which he has devoted his leisure hours for several months, is probably as close an approach to the true path of this body as the observations admit of. A. C. D. C.

MINOR PLANET NOTES.—A Kiel Circular announces that Herr E. von Oppolzer, of Potsdam, has found variations of brightness of Eros amounting to a whole magnitude in a few hours. Prof. W. Valentiner, of Heidelberg, has confirmed the variability by observations with a Zöllner Photometer (*Ast. Nach.* 3687). This would seem to imply that the planet is either not spherical or that its albedo is very far from uniform, and in either case that its rotation is rapid. It will be remembered that similar rapid variability was alleged of many of the older asteroids, but more careful observations did not confirm it.

Many astronomers will doubtless feel some surprise at the low estimate of the value of the present parallax campaign contained in the Harvard Report (quoted in the Feb. number of the *Observatory*, p. 96). It can scarcely be asserted that an object which will give a measurable photographic image in five seconds (vide *Astrophysical Journal* for January, p. 57) is too faint for accurate measurement, either visual or photographic; indeed its brightness is certainly not less, and is probably greater, than those of Victoria and Sappho, from which Sir David Gill deduced his classic value of the solar parallax, while its distance from the Earth was only ⅓ of theirs. It would be too much to assert that the result of the present campaign will outweigh all other parallax determinations combined, but there can be little doubt that it will be more reliable than any other single determination. It will, of course, be far inferior to what it would be at the most favourable opposition, but we shall have to wait 30 years for a better one, and 74 years for the best possible, so there is no question that it is right to utilize the present opportunity.

Ast. Nach. 3687 contains new elements of planet (361) by Dr. Berberich. This planet has the greatest aphelion distance of the entire family, and has not been seen since 1893, the year of its discovery. As the next opposition is a favourable one, its re-discovery is hoped for. The new elements are as follows:—

Epoch	1893 March 12·5.
M	53° 37′
ω	75 47
☊	19 32
i	12 37
ϕ	11 33
μ	450″·4
log a	0·5976

Aphelion distance 4·752
Perihelion distance 3·166
Perihelion distance of Jupiter 4·948

Three new planets have been discovered by Prof. Wolf at Heidelberg, viz., GB on Feb. 15, GC, GD on Feb. 17. GB is of the 11th magnitude and may be identical with (450); the others are of magnitude 13½.

As we go to press we hear of the discovery of three more planets at Heidelberg: GE (mag. 11) by Carnera on Feb. 20, GF, GG (mags. 14 and 13) by Wolf on Feb. 22.

The designation GA is given to a planet discovered by Keeler, at Lick, on 1900 June 28, whose discovery has only just been announced.

The following planets, all discovered by Prof. Wolf and his assistants at Heidelberg, have received permanent numbers (*Ast. Nach.* 3683):—

Planet.	Discovered.	Number.
FJ	1900 Sept. 15	457
FK	Sept. 21	458
FM	Oct. 22	459
FN	Oct. 22	460
FP	Oct. 22	461
FQ	Oct. 22	462
FS	Oct. 31	463

FL does not receive a permanent number, not having been sufficiently observed. FO, FR proved to be identical with (244) Sita, (177) Irma respectively. FQ was identical with the planet DD, discovered in 1896, but not numbered at that time.

Prof. Bauschinger requests discoverers of planets not to delay so long in assigning names to them; the names are a great help to the memory, especially now that their number is growing so immense. Planets between numbers (356) and (443) which have not received names from their discoverers by next July will be named at the Berlin Rechen-Institut. A. C. D. C.

Obituary.—The following notice of the late Mr. Woodd Smith appeared in the *Times* of January 29. It may be added, as showing his interest in astronomy, that he joined the Society in 1861, and was, until quite recently, a very regular attendant at the meetings:—

After an illness of some months' duration, Mr. Basil Woodd Smith, J.P., D.L., who till last summer had lived for many years at Branch Hill Lodge, Hampstead Heath, died early on Sunday morning at Warrior Square, St. Leonards, in his 70th year. Mr. Woodd Smith was a man of many interests. Placed in circumstances which permitted him to give his time and his counsel where it was needed, he was able to assist a number of good causes, and to promote the increase of knowledge in the departments which he had chosen for his own pursuit. At Hampstead, where everybody knew Branch Hill Lodge and many had partaken of its hospitalities, he was a magistrate of long experience and chairman of the bench. The local philanthropic institutions received more than their share of his time, and he was for many years treasurer of the Orphan Working Schools. A man of strong religious convictions, not the less strong because in his life he had been content to learn from many and varied teachers, he found perhaps his chief interest in the work of the Bible Society, and took a scholarly interest in its translation work. With him it was only a step from circulating the Bible to assisting in any enterprise that would substantiate its truth in matters of detail. So he was an ardent member of the Palestine Exploration Fund, like his neighbour, Sir Walter Besant, and of any institution that gave itself to the study of Biblical archæology. He will be much missed also at the Royal Institution, where not only was he a familiar figure at the lectures, but earned and kept his place on the board of managers by his qualities as a good man of business. He was, in fact, a man with many sides to his life, who made friends in many directions.

A New Star.—Edinburgh Circular No. 54, issued by Dr. Copeland on February 25, runs as follows:—

A New Star was discovered in Perseus by Dr. T. D. Anderson, of this City, on February 21, $14^h\ 40^m$ G.M.T. The star was then of the 2·7 magnitude, and shone with a bluish-white light. Dr. Anderson gave as its approximate place for 1901·0

$$\text{R.A. } 3^h\ 24^m\ 25^s, \quad \text{Dec. } +43^\circ\ 34'.$$

At $6^h\ 58^m$ G.M.T. on February 22 the undersigned estimated the Nova as 0·3 mag. brighter than α Tauri, and at $8^h\ 10^m$ considered it equal to Procyon, which it closely resembled in colour.

On February 23, at $8^h\ 10^m$ G.M.T., Dr. Halm and Mr. Clark found the new star 0·2 mag. brighter than Capella.

A direct-vision prism on the 6-inch refractor showed nothing beyond a perfectly continuous spectrum. With the large Cooke spectroscope on the 15-inch equatorial the first impression was the same as with the smaller apparatus; but on closer examination about half a dozen delicate dark (Fraunhofer) lines were made out by Dr. Halm, extending from a little below D to about F. The spectrum of the Nova at its present stage is therefore of a distinct but feebly developed solar type. The existence of these lines I was able to confirm, but the sky became gradually obscured before their positions could be satisfactorily determined.

At about the same hour a photograph of the Nova was secured by Mr. Heath at a time when all the neighbouring stars were obscured by haze. Except a very short interval on the 23rd, the sky here has been completely overcast.

The Nova was independently discovered by Mr. J. E. Gore at Dublin at 11 P.M. on February 22, and by Mr. W. B. Dodd and Mr. H. Wake, of Whitehaven, on the 23rd. RALPH COPELAND.

To this may be added that Mr. Gregg, at Hastings, having seen the Nova on the 22nd, telegraphed to Col. Markwick at Devonport, who estimated its magnitude as 0·5 or 0·75. Mr. S. J. Johnson, of Bridport, also reported independently that he saw it on the same night. The first chance of observation at Greenwich occurred on Monday, February 25, when the sky cleared sufficiently for photographs to be taken. By eye-estimation the Nova appeared as bright as Procyon and of about the same colour. The photographs show from casual examination that its brightness lay about midway between Capella and α Persei. In a letter to the *Times* of Feb. 26, Sir Norman Lockyer states that the spectrum strongly resembles that of Nova Aurigæ; that there are two light sources involved, one with a dark line spectrum, the other giving bright lines of hydrogen, helium, asterium, and calcium; and that the centres of the bright and dark lines are separated by about 15 tenth-metres, showing a differential velocity of somewhere about 700 miles per second between the colliding light sources.

THE HOUR OF MEETING OF THE ROYAL ASTRONOMICAL SOCIETY. —After intermittent discussion extending over more than fifteen years, the Royal Astronomical Society has at last decided to have its Ordinary Meetings in the late afternoon. The history of the movement may be read in full by reference to previous volumes of this Magazine, and is briefly as follows:—In 1885, at the Annual General Meeting, Mr. Sydney Waters proposed that the ordinary meetings should be held at 5 o'clock, or rather, as the proceedings at the Washington Conference were then fresh in the minds of the astronomical public, the words of the resolution ran that "Bye-law 44 be altered by substituting '17 hours' for '8 o'clock.'" The arguments for the resolution brought forward then, as they have been since, were the convenience of members living away from town, and also the impossibility of attending the meetings both of the Astronomical Society and the Royal Institution. The Opposition urged that the desire for a change was not sufficiently general to warrant the alteration, and that it would interfere with the business of the Council. The motion was withdrawn, and Mr. Chambers proposed a resolution, which was agreed to, that post-cards be sent out to all the Fellows, asking their opinion. The result of this *plebiscité*, which was announced at a Special General Meeting called in May for another purpose, showed that 106 Fellows were in favour of 8 o'clock, 125 of 5 o'clock, 150 had no decided preference, and that 17 preferred some other hour. Evidently on the ground that there was not sufficient feeling in favour of a change, a Resolution proposing that the hour be altered to 5 o'clock was lost. The question was next brought forward at the Annual General Meeting in 1890, when a resolution standing in Mr. Brett's name was introduced in his absence by Mr. Chambers, to the effect that meetings should take place at 4 o'clock. Dr. Common proposed as amendment that 5 o'clock should be substituted for 4 o'clock; and this being carried, Mr. Knobel, seconded by Mr. Mitchener, proposed that the question be deferred for consideration by the Council, which was agreed to after a discussion on the usual lines, Capt. Noble adding that business men might not be able to attend in the afternoon, especially on Fridays, which was often the night for making up the Indian mail.

In pursuance of the resolution the matter was considered by the Council, but the next public step taken was by Mr. Chambers, at whose instigation a Special General Meeting was called for 1890, Dec. 12, when he proposed that the hour of meeting be $4^h\ 30^m$ *for a period of one year.* As it was ruled that the italicized words could not form part of a resolution, these were withdrawn, and, on the proposal of the Astronomer Royal, the time $4^h\ 30^m$ was altered to 5 o'clock; but this altered Resolution did not meet with approval, and was rejected by 30 votes to 18. Mr. Chambers asked that a poll should be taken by post-cards; but this was refused, as being contrary to the bye-law, which states that bye-laws can only be altered at the Annual General Meeting or at a Special General Meeting: nevertheless, Mr. Chambers, after due notice, raised the question again at the General Meeting in 1891, February, a procedure which received some criticism, one speaker characterizing it as "indelicate," since it re-opened a question which had so recently been adjudicated on. The motion was lost by 4 votes, 31 persons voting for and 35 against. Dr. Schuster proposed an amendment that the meetings should be alternately in the afternoon and in the evening; and Mr. Petrie made a suggestion in opposition to the original motion, which will now be put to the test, namely, that though 5 o'clock may be a convenient hour for the attendance of business men, if they come unfed, straight from a hard day's work, they will not be so fit to appreciate the points of the papers read and so will not get due enjoyment out of the meetings.

The subject then dropped for a year or two, but at the General Meeting of 1894 again Mr. Chambers had a resolution on the agenda paper, although he was not present to propose it, and the duty fell on Mr. Waters, as he was the original proposer of the change. The question of other hours was discussed, the poll by post-card was suggested, the successful meetings of the B. A. A. adduced as reason for an afternoon meeting, the trite arguments again brought forward, but still the meeting negatived the motion that the meetings be held at 4.30 P.M., although, on this occasion, the Council yielded the point that the change would not interfere with their business. The next occasion when an attempt was made to alter the hour was last year (1900), when Mr. Mitchiner, who, it will be remembered, opposed the change on an earlier occasion, wished to bring forward a resolution, but which, for certain reasons, was taken out of his hands by the Council, to the effect that the hour of meeting should be 5 o'clock: the reason why this question was not actually put to the vote, and why it has been brought forward again this year, will be found in the Report of the last Meeting given in this number. It will shortly be seen what effect this long-debated alteration will have upon the attendance at Burlington House, and on the style of the meeting, but it is worth putting on record that the question was decided at a fully representative meeting; a large number of Fellows were present, many, or possibly the majority, being of those who attend the evening meetings, and the motion was carried almost unanimously.

THE LENGTH OF THE SUN-SPOT PERIOD.—Prof. Newcomb is an astronomer who touches all branches with equal facility. In the January number of the *Astrophysical Journal* he has a neat discussion of the figures relating to sun-spots, from which he deduces a period and other interesting facts. About periodic phenomena, says Prof. Newcomb, two hypotheses can be made: one, that similar phases recur at equidistant periods of time, subject only to variations of the nature of accidental errors; and, secondly, that there may be a normal mean period, but that variations are cumulative—that is to say, if a phase is once accelerated this advance will go into all subsequent phases. If the irregularity be represented by a quantity $\pm\epsilon$, and P be the true value of the normal period, on the first hypothesis the occurrence of a phase after n periods will only differ from time nP by a quantity $\pm\epsilon$, whereas on the second hypothesis the probable deviation from the normal would be $\pm\epsilon \times \sqrt{n}$, and would increase with n.

With the dates of the observed maxima and minima of sun-spots from 1615 to 1893 to work with, these hypotheses are put to the test: it may be remarked in passing that the data are derived from Rudolf Wolf's figures up to the year 1872, and after that from the Greenwich observations; and that besides dates of maxima and minima, the dates of two other phases of the period are derived from the same sources. However, with a provisional period of 11·13 years, epochs for these four phases are computed and compared with the observed epochs, and the differences of the computed and observed dates show: (1) that there is no accumulation of variation in the sun-spot period, but that the first hypothesis more nearly fits the case; (2) that certain apparently large departures in the period about the years 1670–1680, and again from 1780–1790, are due to imperfection of the record; (3) the mean period is 11·13 years, with a probable error of $\pm 0^{\mathrm{y}}{\cdot}02$, and that the mean period from minimum to maximum is 4·62 years, and from maximum to minimum 6·51 years. Finally, Prof. Newcomb sums up in these words:—"Underlying the periodic variations of spot activity there is a uniform cycle, unchanging from time to time and determining the general activity"; and adds, for convenient reference, the following dates deduced from his research:—

Maxima.	Minima.	Maxima.	Minima.
1871·52	1878·03	1904·91	1911·42
1882·65	1889·16	1916·04	1922·55
1893·78	1900·29	1927·17	1933·68

Dr. See has now published his paper on the dimensions of Saturn's rings and diameter in *A. N.* 3686–87, and he is to be congratulated on securing so excellent a series of measures with the 26-inch. During the short time he has been in charge of this instrument he has turned out some most valuable work.

In the course of the paper Dr. See acknowledges the willing help given by Mr. George Anderson, who has been the right-hand man of observers with the 26-inch for 27 years. We notice with regret that the same issue of the *Ast. Nach.* reports his death. Mr. Anderson was born in Scotland in 1839, and served in the Union Army during the War of the Rebellion. He discovered some close double stars and faint companions to α Lyræ, A Leonis, and other bright stars.

The Weather in February.—During the greater part of the month the weather in February was cold, the mean temperature from February 1 to 21 having been 33°·5, which is lower than the average value by 5°·9. The lowest temperature occurred on the 14th, being 20°·4, and the mean for that day was as much as 11°·6 below the corresponding average. The weather was milder during the latter part of the month, the mean for the seven days February 22–28 being 2°·7 above the average. It has been a very cloudy month, and sunshine has been very deficient, the recorded sunshine for the month amounting to 27 hours only, which is less than 10 per cent. of the possible duration. So small an amount has not been recorded in February since 1888.

Rain or snow to measurable amounts fell on 11 days in the month, producing a fall of 0·86 inch, an amount which is less than the average for the preceding 60 years by 0·64 inch.

A Correction.—An unfortunate mistake occurred in our last number, which we wish to take all possible steps to rectify. In the abstract of Mr. Evershed's paper on p. 83, line 5, the word "not" was put in by the printer and the mistake was passed in reading the proof. As this obviously reverses Mr. Evershed's conclusions, will readers kindly obliterate the word "not" in their copies?

We see from an announcement in the daily press that the famous observatory on the Seeberg, near Gotha, which was the scene of the labours of the famous astronomers Encke, Zach, and Lindenau, was burned down on the night of Tuesday, Feb. 19.

We sometimes express a polite incredulity as to the methods and results of astrologers and such folk, so that it seems only fair to state, without any intention of expressing an opinion, that in

Old Moore's Almanac for the current year, among the predictions for January, it is said: "An astronomical discovery of the first importance will be welcomed about this period, and a fortunate scientist will receive his reward from the hand of Royalty." Now, who is the scientist?

It is stated that Mr. C. T. Yerkes, the founder of the Observatory at Williams Bay which bears his name, is coming to live in London for a time, perhaps permanently.

Arrangements are being made by the Royal Academy of Sciences of Sweden to celebrate the third centenary of the death of Tycho Brahe by a Special Meeting on 1901, Oct. 24.

It is stated that Mr. W. H. Crocker, of San Francisco, has offered to defray, as he has done before, the expenses of an expedition from the Lick Observatory to Sumatra, to observe the coming eclipse of the Sun.

The Isaac Newton Studentship in Physical Astronomy at Cambridge has been awarded to Mr. S. B. McLaren, of Trinity College.

The next Meeting of the Royal Astronomical Society will be on Friday, March 8, at 5 o'clock in the afternoon. Tea will be served in the Library at 4.30. The next meeting of the British Astronomical Association will be on March 27.

From an Oxford Note-Book.

The Report of the Director of the Harvard Observatory is always a striking document. We always learn of vast quantities of work completed, being done, or projected; but this year some of the paragraphs approach the sensational. Firstly, the Institution is in sore need of money. "It will be seen that the sum of half a million dollars is required to enable this Observatory to maintain its place among the greatest observatories in the world." The Director asks for this sum with the *sang-froid* of a man who generally gets what he wants, though it makes some of us in Europe open our eyes. Next we are startled to read that "The recent action of the Corporation in providing for the compulsory retirement of its officers on account of age renders it especially important that unfinished work should be completed rapidly." Are we really and seriously to contemplate the possibility of Harvard Observatory without Professor Pickering? We earnestly trust that the realization of this vision is far off. But the possibility of it has led to a general stock-taking of the unfinished work in hand; and what does anyone suppose is the total? How many more volumes of Harvard Observations will it take to hold the work already in hand, much of it nearly complete? The answer takes one's breath away. *Twenty-eight volumes*, or about two-thirds as many as have been published during the half-century of the existence of the Observatory! Comment is not only superfluous, but well nigh impossible.

The Report of the Superintendent of the U.S. Naval Observatory furnishes a strong contrast to that of Prof. Pickering. It will be remembered that a Board of Visitors of the Naval Observatory was appointed recently, and in their first report they recommended some sweeping changes in organization; in which they probably have the approval of all who are not directly interested, and who know anything about the matter. That the head of a great national observatory should be changed every two or three years, and should be a naval officer with a rather elementary knowledge of astronomy, is an anomaly which cannot be for the good of the place. The Washington Observatory has done excellent work in spite of it, but this does not justify the system. However, the Superintendent himself cannot, of course, be expected to acquiesce in the recommendation of the Board for abolishing him, and in his Report he does his best for himself and his predecessors by replying to the report of the Board. The discussion and decision must be left to our friends over the water: we can only hope that the fight may not be prolonged, for the observatory cannot be expected to do its best work while such vital questions of constitution remain unsettled. As it is, the publications have fallen a good deal into arrear, the volume for 1891 being still in the printer's hands, "awaiting some changes in, or additions to, the Appendix, which deals with the 1874 Transit of Venus Observations." But the present "Astronomical" Director is apparently bestirring himself to catch up these arrears, and we may wish him all success.

IN the account given last month of the Proclamation of King Edward VII. there is an unfortunate inaccuracy which I should wish to correct. It is therein stated that the Vice-Chancellor gave three cheers for the King. I have since seen the official account of the proceedings in the *University Gazette*, and I find that what actually occurred was that "Mr. Vice-Chancellor gave the signal for acclamation." It only shows how mistaken one can be in apprehending things which passed before one's own eyes and ears.

THOSE who go to Mauritius for the eclipse of May next should consider the advisability of subscribing to the Hindoo temples in order to increase their chances of fine weather. In the *Planters' and Commercial Gazette* for December 18 last there is a front-page advertisement of the Marday Veeren temple, which opens with reproaches for past neglect, gives proofs of present attention to business, and ends with an appeal for financial aid. The business announcement loses some of its force by being made two days after the heavy rain had actually set in; but the appeal for help is pathetic.

> Religious ceremonies having been carried out at Marday Veeren temple in the above said place, for about forty five days commencing on the 10th December 1899, it was gazetted on the 15th January 1900, that rain with thunder storm will fall on the next day, and no one seemed to take notice of the fact. Again on the 26th July 1900, notice was given to the effect that any one who was in need of rain, may apply to the president of the above Hindoo temple who will pray God for favour of rain, and for this also no one took care.

That on account of being no rain for the last several weeks from the 16th November till Friday the 14th December 1900 religious ceremonies and prayers were carried out in our said temple our God has revealed the secrecy to me the undersigned that the whole island will be favoured with good rain with thunder bolts and darkness from the said Friday the 14th till the Thursday the 20th instant and that on the next Friday the 21st instant there will be a good sunshine.

That as the above said facts are presumed to take place on the said dates and as the Government of this island does not take care for paying to the above Hindoo temple any part of the yearly expenses and of any necessary important work connected with the above said temple, as God is general to every set of religions to which the whole population belong, the Government and almost all the proprietors of the Sugar Estates are earnestly requested to render some assistance to the above said temple and we also pray our God will be so kind as to make this colony flourish with seasonable rains and good crops.

By order of
THANDAVARADOOM RUTHRAMOORTEE.
GOD.
(Sd) V. NARAYNNASAMY PILLAY.

No. 12, Bourbon Street, Port Louis.
17th December 1900.

OF recent paradoxical literature a small pamphlet by W. H. A. Wallinger * is worthy of the shilling asked for it. The author believes the Earth to be an "isolated gaseous balloon floating in the air," the crust being very thin ; and he is appalled at the rate at which this crust is being destroyed by miners. "Our present destructive work could not be more distinctly fatuous, more dangerously evil, and more perilously suicidal, if we had another well-preserved globe in close propinquity to resort to in case of an accident to this one." This is an odd argument; there seems to be a "not" left out somewhere. But there are plenty of bewildering sentences. "On this globe, insectivorous, and indeed other and larger animal life, as birds, could be placed." "The apparent 'Dip' of the Sun at noon, is simply the focussed light perforating the circumambient vaporous atmosphere with the parallactic angle reduced to a minimum. It would now seem expedient to, at once, plant belts of the *Sequoia gigantea* on the 23½° N. and S. lines of latitude. Thus would there be existing, for five or six thousand years to come, most natural landmarks for determining the alteration in the position of the Earth." There is plenty more of this sort, all for 1*s*.

Q.—THERE is reason to believe that the Earth now rotates in a longer time than formerly. What effect would such alteration have upon the length of a second of time ?

["A silly question," writes the sender, "but I did not set it." The point of the question lies in the following two amusing answers to it.]

Answer (*a*).—The Earth turns round more slowly now than it used to because it has become so densely populated.

Answer (*b*).—The fact alluded to in the question probably accounts for the great ages of the people in the Bible, since the Earth moving more slowly the years have become longer.

---

* 'The Earth : floating in the Atmosphere.' By W. H. A. Wallinger, Late Deputy Conservator of Forests, Bombay. London : William Whiteley, Douglas Place, Queen's Road, Bayswater. 1899.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 March 8, 5 P.M.

Dr. J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., and E. T. Whittaker, M.A.

The Minutes of the January Meeting were read and confirmed.

Mr. Whittaker. One hundred and two presents have been received since the last Ordinary Meeting. Among them are volume 8 of the 'Edizio Nationale Galileo Opere,' presented by the Italian Government; the volume of the zone (—2° to +1°) of the 'Astronomische Gesellschaft Catalogue' made at Nicolajew; the Astronomical Observations of Nansen's North-Polar Expedition, arranged and reduced under the supervision of Prof. Geelmuyden, and presented by him to us; 'Potsdam Astrophysikalisches Observatorium, Photographische Himmelskarte,' Band 2, presented by the Observatory; and Prof. Turner has given the Society a copy of his book, 'Modern Astronomy.' We have also received lantern-slides from photographs of the Total Solar Eclipse of 1900 May 28 from Count de la Baume Pluvinel.

The President. We have received a number of communications on the subject of the new star in Perseus, which we will take first. The first paper is by Mr. Stanley Williams.

Mr. Dyson read the paper, which stated that on February 20, at $10^h\ 42^m$, Mr. Williams obtained a photograph of the region in Perseus where the Nova was afterwards seen. This distinctly showed stars of the 10th magnitude, but did not show the Nova, which must therefore have been fainter than magnitude 10 at this time, only 28 hours before the star was seen by Dr Anderson.

The President then called upon Mr. Newall.

Mr. Newall. I shall be very glad to make my communication about the Nova, but it refers to rather a special part of the spectrum, and it seems to me that it would be better if we could see some examples dealing with the whole range of the spectrum, and then pass on to details.

The President assented to this course and called on Dr. Lockyer.

Dr. W. J. S. Lockyer. After the discovery of the Nova on February 22 by Dr. Anderson, and the telegram received from Dr. Copeland on the afternoon of the same day, no photographic work was possible until the evening of the 25th, owing to the cloudy weather. An occasional glance, however, showed that the star had considerably brightened, and on the 25th it was about of the 1st magnitude. It is interesting to note that the star as described by Dr. Anderson was of a bluish-white tinge, while on the 25th it was of a decided red tinge. The position and photographic brightness of the Nova as compared with the stars in the same constellation will be seen from the photographs on the screen.

With the three instruments in regular use at the Solar Physics Observatory, South Kensington, namely, the 6-inch prismatic camera with one prism of 45°, the 30-inch Common reflector with a slit spectroscope of two prisms, and the 9-inch prismatic reflector with one prism of 17°, numerous photographs were obtained with varying exposures. The general appearance of the spectrum was a set of dark hydrogen lines associated with bright broad components on the less refrangible side, and several other bright bands with possibly dark components on the violet side. The spectrum resembled generally that of Nova Aurigæ.

One of the chief features of the principal bright lines is their great width, amounting to about 30 tenth-metres, and the dark components. With comparison spectra of γ Orionis or α Persei, photographed on the same plate as the Nova, the middle of the bright lines was found to be not far from the normal positions; the dark lines were displaced some 15 tenth-metres towards the violet, thus indicating a differential movement of something like 700 miles per second. Movements, therefore, more rapid than those in Nova Aurigæ were taking place.

In the spectra secured with the 6-inch prismatic camera the structure of the bright broad bands was well brought out. An interesting feature was the presence of fine dark lines down the middle of the bright lines of hydrogen and calcium; these are probably reversals. $H\gamma$ and $H\beta$ are also probably reversed.

Since the 25th, further photographs have been secured on February 28, March 1, 3, and 5. It will be seen from these slides shown on the screen* that the spectrum was undergoing rapid changes, as the brightness of the star was diminishing. Visually the hydrogen lines were distinctly bright, C being most brilliant, and F following closely after. The spectrum on February 28 was brighter in the ultra-violet than had previously been noticed. The changes in the other bright bands were also of great interest, these apparently gradually becoming less intense as the hydrogen lines grew in brilliance.

---

* A set of photographs were thrown on the screen showing the spectrum of the Nova and of α Persei for comparison, taken on the nights of February 28, March 1, 3, and 5. These spectra, arranged vertically one under the other, showed at a glance the changes the spectrum is undergoing.

As it was ascertained that the spectrum was similar in general appearance to that of Nova Aurigæ, and the chief lines in Nova Aurigæ, as recorded by Campbell and others, were coincident with most of the chief lines in the spectrum of α Cygni, a comparison of the Nova with the spectrum of α Cygni was undertaken. Allowing for the fact that the lines in the Nova were all broad and diffuse, and that groups of narrow lines would in such a spectrum be represented by bands, it was found that there was a great likeness between the two spectra. In fact, most of the strongest lines of iron, enhanced lines of iron as they are in α Cygni, had their equivalents in the Nova spectrum. The stage of temperature thus reached was not, comparatively speaking, very high.

As far, then, as it can be determined, the spectrum of the Nova consists of the lines of hydrogen, calcium, and the enhanced lines of metals, chiefly iron, and it will be most interesting to watch the changes as the temperature is reduced. The star is rapidly on the wane, so it is hoped that the weather will not prove unfavourable to observation.

Mr. McClean. I was able to observe the spectrum of Nova Persei visually on the 25th February. Photographs of the spectrum were obtained by me each night from the 27th February to the 3rd March, and again on the 5th and 6th March. They were taken with an object-glass prism of 20° angle, attached to a refracting telescope of 12 inches aperture.

The lantern-slide shown is taken from the spectrum of Nova photographed on the 3rd of March. It shows the diffuse dark bands, other than those of hydrogen, with the greatest distinctness they have attained to.

The sheet of mounted enlargements exhibited was made from the spectra of Nova taken on the 27th February and on the 3rd March respectively. The spectra of β Crucis and of Sirius are mounted beside them for comparison. The hydrogen series of lines ($H\beta$ to $H\epsilon$) are at once recognized, both as absorption lines or bands, and also as bright emission bands. The bright hydrogen bands are very broad (between 30 and 40 tenth-metres), and are displaced just clear of the corresponding absorption lines towards the red.

The generally accepted theory is that the relative displacement of the bright and dark lines is due to the difference of velocity in the line of sight of the respective sources of light; but this does not account for the great width of the bright hydrogen bands, neither does it account for the apparent recurrence of the displacement of the bright bands towards the red in the spectra of α Aurigæ and of Nova. The displacement of the bright bands ought to take place as often in one direction as the other.

With regard to the other lines in the spectrum of Nova, the comparison with the spectrum of β Crucis, which is a characteristic helium star, shows that the presence of helium in Nova is very doubtful.

The calcium band (K) is present, and other absorption bands and lines are present, apparently due to titanium and calcium. There is also a certain amount of correspondence between the dark bands in Nova and the groups of lines in Sirius, which suggests that Nova may belong, at present, to the class of Sirian stars.

Mr. Newall. On February 25 I was able to get a glimpse at the Nova, but only through a thick cloud. Photographs of the spectrum were obtained on other nights, February 26 and 28, and March 3 and 5. In the F lines there are many alternations of brightness. The displacement of the lines tends to show that the star is moving towards us *.

The Astronomer Royal. Observations have been made at Greenwich by various observers, and photographs have been taken, mainly with a view to ascertaining the position and magnitude of the Nova, specimens of which, showing images of the Nova and of comparison stars side by side, are shown on the screen. The star was first seen on February 25, when the mean of the eye-estimations by several observers gave 0·5 as its magnitude. Similar observations on February 27 gave 1·8, on February 28 1·75, on March 1 2·2, on March 2 2·1, on March 3 2·2, on March 5 2·7, and on March 6 3·1. Attempts have also been made to photograph the spectrum with the slit spectroscope on the 30-inch reflector, but only faint spectra have been obtained.

Dr. Rambaut. On the receipt of the telegram announcing its discovery preparations were made at the Radcliffe Observatory for observing Dr. Anderson's new star in Perseus, but owing to the cloudy weather that prevailed it was not seen by us until the evening of the 25th February. At this time its magnitude appeared to be 0·9.

The Oxford observations include transit-circle observations of the position of the star, measures of its brightness made with a wedge photometer, a spectroscopic examination, and eye-estimates of its brightness as compared with several of the bright stars. The present notice is chiefly concerned with the eye-estimates of magnitude. In these comparisons the magnitudes of the Harvard Photometry have been adopted, and the observers have estimated the difference between the Nova and each comparison star in tenths of a magnitude. In these estimates, of course, discrepancies occur, but they are much less than might have been expected, and the general agreement of the individual results is remarkably good, as may be judged from a particular instance selected at random. Thus, on the night of February 25, the separate determinations of magnitude made by Mr. Robinson and Mr. Wickham were 1·0, 0·8, 0·9, 0·8, 0·7, 1·0, 0·9, 0·7, 0·6, 0·9, 1·1, and 1·0. [A slide was here thrown on the screen, giving the results of the observations.] We have here the results for each evening as deduced from a large number of observations by four observers—Mr. Wickham, Mr. Robinson, Mr. McClellan, and myself,—from which it may be seen how the light fell away from the time we first saw it. The magnitudes on the several dates were as follows:—

---

* [Mr. Newall explained some details of the spectrum. We regret that owing to his departure for Sumatra soon after the Meeting we are unable to give a more complete account of his remarks.—Eds.]

Feb. 25....	0·90	Feb. 28....	1·58	Mar. 3....	2·20
26....	1·17	Mar. 1....	1·97	5....	2·42
27....	1·51	2....	2·08	7....	2·95

On the same diagram are indicated the relative intensities of the light, from which it will be seen that between February 25 and March 6 the star lost about 85 per cent. of its light. I have to add that since the preparation of this table and diagram the star seems to have actually brightened a little, last night (March 7) Mr. Wickham and Mr. Robinson independently estimating it at 2·75 and 2·78 respectively.

On the nights of February 26 and March 5 the star was examined with a single-prism spectroscope attached to the Barclay 10-inch equatorial. I bring these spectroscopic observations forward with considerable diffidence in view of the beautiful photographic spectra which we have seen here this afternoon, but I hope that, as eye-observations, they may have a certain value of their own. On February 26 we found a bright continuous spectrum on which were superposed several bright lines. The most conspicuous of these was near the extreme red end of the spectrum and was taken to be C. There was also a faint line in the orange (D ?), and two in the green. The more refrangible of the latter was taken to be F or possibly Hγ. There was strong absorption in the orange and green.

On March 5 the general appearance of the spectrum was very similar. The absorption above C (?) and F (?) was very strongly marked. The C line was relatively much more brilliant than on February 26, which may account for the increased redness in the light of the star.

Mr. Bellamy exhibited photographs of the field around the Nova showing stars of faint magnitude which could be used for comparison if the Nova becomes of the 9th or 10th magnitude.

The Astronomer Royal. Mr. Bellamy's remarks remind me that I omitted to show a photograph which may be of interest. It is a photograph of the Nova and the field surrounding it, which can be measured to determine its position.

Captain Noble. I should like to say that I was struck with the curious disproportion between the light of the spectrum and the relative magnitude of the star. I adjusted a spectroscope on Capella on the night of March 3, but upon turning it upon the Nova I was surprised to find the spectrum so bright, as the star was so much fainter to the eye than Capella. Of course, the spectrum was more variegated and would therefore be more conspicuous, but it was actually, and not relatively, brighter than that of Capella. I estimated the magnitude of the Nova as exactly equal to that of Algol, and I was pleased to see the Astronomer Royal write 2·2 against that date on the blackboard.

Mr. Seabroke. It seems to me that the peculiarities in the spectrum may be explained without assuming such great velocities. May not the dark bands be partly hidden by the bright ones?

The President. I am sure the Society will return their most cordial thanks to the authors of the papers we have heard this evening. They have already done so by their applause. It is satisfactory to find that the National Observatory, the Observatory at Cambridge, and the Oxford Observatories have already sent in their observations.

Mr. Bryan Cookson then gave a description of a floating photographic zenith telescope, and some results obtained with it at Cambridge.

Sir Robert Ball. We at the Cambridge Observatory are very proud of this piece of work which Mr. Cookson has described. Mr. Cookson undertook the designing and carrying-out of this instrument as a piece of work necessary for obtaining his degree. He has obtained the high approval of those who have had the privilege of seeing this paper. The instrument has already given earnest of what it is capable of, and we now give Mr. Cookson our best wishes on his starting for the Cape, where he is to have the inestimable advantage of working for two years under Sir David Gill.

The President. You have already cordially thanked Mr. Cookson for the communication he has made to us this evening. I understand that he sails for the Cape to-morrow, and I am sure we shall all wish him a good voyage and success in the work he is about to undertake.

Mr. Wesley then showed on the screen some photographs of the eclipse of 1900 May. One photograph of the corona, taken by Count de la Baume Pluvinel with a Cooke 5½-inch lens with 20 seconds exposure on a rapid plate, was particularly interesting.

The following papers were announced and partly read:—

W. E. Cooke. "Partial Solar Eclipse, 1900 November 22, observed in Western Australia."

R. T. A. Innes. "Observation of Position-angle of Polar Double Stars."

H. H. Turner. "The Oxford Photographic Determinations of Stellar Parallax; Reply to the Criticisms of Sir D. Gill."

J. Tebbutt. "Occultation of Jupiter and his Satellites, 1900 September 29."

Royal Observatory, Cape of Good Hope. "Cape Double-Star Results, 1900."

Royal Alfred Observatory, Mauritius. "Observations of Leonids, 1900 November 15, 16."

Bryan Cookson. "Description of a Floating Photographic Zenith Telescope, and some Results obtained with it."

H. C. Plummer. "Note on Mr. Cookson's Paper 'On the Accuracy of Eye-observations of Meteors, &c.'"

A. W. Roberts. "The Variable Star R Centauri."

A. Stanley Williams. "On the New Star in Perseus."

J. L. E. Dreyer. "Further Corrections to the Armagh Catalogue, with special reference to the 'Anonymous' Stars."

Rev. W. Sidgreaves. "Note on the Spectrum of Nova Persei observed at Stonyhurst College Observatory."

F. A. Bellamy. "Positions of Nova Persei and 159 Stars within 25′ Distance from it, from a Photographic Plate taken at the University Observatory, Oxford."

F. McClean. "Nova Persei."

Radcliffe Observatory, Oxford. "Observations of the New Star in Perseus."

The following gentlemen were elected Fellows of the Society:—

R. W. Chapman, M.A., B.C.E., Lecturer in Engineering and Physical Science, University, Adelaide, South Australia.

Chas. J. Isaac, Head of the Upper Nautical School, Greenwich, S.E.

The following Candidates were proposed for election as Fellows of the Society:—

J. S. Chakravarti, M.A., Assistant Accountant-General, Rangoon, Burma (proposed by P. Doyle).

C. S. Mence, Teacher of Navigation and Nautical Astronomy, 49 Watling Street, E.C. (proposed by V. L. D. Broughton).

Rev. John Stutter, O.S.B., Acton Burnell, near Shrewsbury (proposed by A. C. D. Crommelin).

E. G. Wainwright, B.A., Lecturer in Mathematics and Physical Science, St. John's College, Battersea, S.W. (proposed by A. E. Moore).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE fourth Meeting of the current Session of the British Astronomical Association was held at Sion College on Wednesday, the 27th February, *Mr. G. M. Seabroke*, President, in the Chair.

The names of nine Candidates for Membership were read and passed for suspension, and the election by the Council of one new Member was confirmed.

The President announced that the Council had that afternoon given its assent to the formation of a Midland Branch, to include the counties of Warwick, Stafford, and Worcester.

Colonel E. E. Markwick, Director of the Variable Star Section, read a paper on the Nova Persei of 1901. He said that on Friday evening, the 22nd February, he received a telegram from Mr. Gregg, of St. Leonard's, a member of the Variable Star Section, reporting a strange star to the left of Algol. Stepping to the front door, which faced west, he at once noticed a brilliant star, above and to the left of Algol, which must be new. To the naked eye it was white, and at 9.12 its magnitude was estimated, by comparison with *α* Persei and Capella, to be 0·72. At 11 o'clock he studied it carefully, and estimated its magnitude at 0·90. He subsequently made out its position to be for 1900 (approximately) R.A. $3^h\ 23\frac{1}{2}^m$, Dec. $+43^\circ\ 37'$. The following evening, February 23, it had undoubtedly increased in brilliance. Three comparisons with Capella, *a* Persei, and Procyon gave 0·37, 0·25, and 0·34. Further observations on the 25th and 26th pointed to the conclusion that it had attained its maximum brightness on February 23.

Mr. Gregg contributed a short paper in which he said that he first observed the star on the evening of February 22, about 15 hours after Dr. Anderson first saw it in Edinburgh. When first seen it was slightly below the first magnitude, being brighter than Aldebaran, and but very little below Rigel in magnitude. It shone with a steady bluish-white light. On Sunday, the 24th, it had increased in brightness, being then brighter than Rigel. On the 25th it was still a brilliant object, above the first magnitude.

Mr. W. T. Lynn said that, owing to the bad weather, probably few people in London had seen the star. He was fortunate enough to see it on Monday evening at 10 o'clock. He was somewhat disappointed with the appearance of the star, which he had expected to be brighter. To his eye it was not only considerably fainter than Capella, but less bright than Castor and Pollux.

Mr. R. F. Rendell said the star was observed with the transit-circle on February 25, when it passed the meridian about 5 o'clock. At about 6.15 it was quite visible to the naked eye, and appeared distinctly fainter than Capella. It seemed to him to have faded a little in brightness towards midnight, but that might be due to difference in altitude.

Mr. Crommelin said that at Greenwich they first heard of this star between 1 and 2 o'clock on Friday afternoon, when the Astronomer Royal received a telegram from Dr. Copeland stating that Dr. Anderson, who, it would be remembered, discovered a new star in Auriga some nine years ago, had detected this object about 2 A.M. that morning. The star would have been very low down then in the N.W., which made the discovery the more remarkable. He believed that Dr. Anderson knew the sky so well that he could detect a new 5th-magnitude star in almost any part of the heavens. There were very few of whom that could be said. Knowing the sky so well he certainly deserved his good fortune in being, so far as they knew, the first to make this most interesting discovery. The weather prevented any observations in England until Monday the 25th. The position given by Dr. Copeland was found to be very nearly correct. It was observed on the meridian that night, R.A. being $3^h\ 24^m\ 28^s{\cdot}21$, and N. Dec. $43^\circ\ 33'\ 54''{\cdot}8$, referred to the equinox of 1901·0. The 'Bonn Durchmusterung' gave no star in that plane. The nearest star was the 9th-mag. star +43° No. 739, and this was probably the one Col. Markwick referred to. It was estimated at Greenwich that the magnitude was fainter than Capella, but brighter than most of the other stars about—certainly brighter than any of the Perseus stars. Roughly it was about a standard first-magnitude star on Monday night.

Mr. Whitmell said that the Rev. S. J. Johnson saw the star on Friday night almost contemporaneously with Col. Markwick.

In reply to a question by the President, as to whether any Members had observed that region of the sky recently, Col. Markwick said there was certainly no star of the first or second magnitude there on February 15.

Mr. Maunder said that Prof. Pickering had photographed the region of the Nova on February 19 at the Harvard College Observatory, but the photograph showed no trace of its presence. The star must therefore have been fainter than the 11th magnitude on that date.

Mr. Holmes asked if the spectroscope had been applied to the star at Greenwich.

Mr. Crommelin said he believed that the instrument was not in adjustment on Monday night.

Dr. Downing said that in a very interesting communication to the *Times* Sir Norman Lockyer stated that he observed the star with a spectroscope at South Kensington, and found the spectrum to be very similar to that of Nova Aurigæ. It showed a continuous spectrum of the solar type, with dark lines, and superimposed upon that a gaseous spectrum, with bright lines of hydrogen and other gases, so that Sir Norman Lockyer seemed to think that this object was in a similar state to that in which Nova Aurigæ was when it was at its brightest.

Mr. Holmes read a paper on "Communications with Mars." He said what the future had in store we could not know, but he thought it safe to assert that not only did we know nothing about life upon Mars, but that no human being had any idea, which would bear examination, of any means of knowing anything about such life at the present time. There was an inherent improbability on the evolutionary theory that two planets should, at the same epoch, be in the same stage of development, and especially that the smaller and more distant should be at the same point in its progress as the larger, and nearer the Sun. But waiving this, Mars might be swarming with life, and yet none of it of an intellectual character. Even if Mars were the abode of forms of life equal to the human race in intellect, we had to remember that the human race had certainly existed many thousands of years, and that it was only in the last few hundred that even the most advanced had attained any knowledge of physical astronomy. The matter of signalling had usually been treated as if the signals would be made when Mars was in opposition; but in this position the Earth was invisible from Mars, and also for a considerable time before and after. By the time the Earth was sufficiently visible from Mars, about fifty million miles must separate the two. Mr. Holmes then, in a somewhat humorous manner, illustrated the impossibility of contriving any intelligible system of signals, even assuming that intelligent beings existed to make and receive them.

Mr. Whitmell read a paper on the occultation of Saturn on September 3, 1900. He referred to valuable communications from Mr. Newbegin, Dr. Smart, and Mr. Adams, recently published in the *Journal*, and expressed his great indebtedness to Mr. Crommelin for data connected with Greenwich and Toulon. He said the problem of the occultation of an ellipse by a circle was a very interesting one, and this particular problem was complicated by the lunar parallax, and by the fact that the major axis of Saturn's ring was placed unsymmetrically in relation to the tangents at the Moon's limb, at the points of disappearance and reappearance. Remarkable differences between the duration of immersion and that of emersion were shown to occur, and the actual duration of an immersion or an emersion was largely increased to observers in latitudes near the southern limit.

Mr. Crommelin said he thought all present would feel much indebted to Mr. Whitmell for dealing in such an interesting way with a subject which was so difficult and so technical. The occultation of an oblique ellipse by the Moon was the most puzzling case in occultations, and the subject occupied a great many pages in Chauvenet's 'Astronomy.' It was a very complicated problem, and Mr. Whitmell's graphic method of dealing with it was not only the shortest, but also the easiest one to understand.

Mr. Newbegin said it was very gratifying to find that the observation he made at Norwich was so well confirmed by the calculations which Mr. Whitmell had made.

Mr. Rendell remarked that it was practically impossible to get an accurate observation of the reappearance of Saturn. A large part of the ring had emerged before the observer realized it, and the belt itself was so shadowy and vague that he did not think the observation was very trustworthy. The disappearance was very good, but he did not think anything could be built up on the observation of the reappearance.

Mr. Goodacre described and showed, with the aid of the lantern, some views of a simple and inexpensive observatory which he had erected, the materials for which could be obtained at a cost of from £3 to £4.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, March 20, at the Institution of Civil Engineers, Westminster, *Mr. W. H. Dines*, President, in the Chair.

Dr. Hugh Robert Mill delivered a most interesting lantern lecture on "Climate and the Effects of Climate." Climatology is as much a branch of geography as of meteorology, in fact more, for it not only deals with the distribution of atmospheric conditions over the Earth's surface, which is a geographical question in itself, but all the varieties of climate that give individuality to different countries are produced by the disturbing or controlling influence of land-forms. It was while studying the influence of land-forms on every kind of geographical distribution that Dr. Mill was struck by the far-reaching interest of the effects of climate; and so in this lecture he dealt with the visible effects of climate, such as attract attention and arouse enquiry as to their causes. After making a few remarks on the principles of scientific photography, and also calling attention to spurious photographs, the lecturer proceeded to distinguish between weather and climate. Weather is the condition of the atmosphere at any moment with regard to wind, warmth, cloud, electricity, and precipitation, whilst climate may fairly be called the average weather of a place. Dr. Mill then exhibited on the screen a large number of photographs which he had himself taken on various holidays in many countries, in order to illustrate the peculiarities of climates in which heat, cold, wind, and rain respectively predominate, showing how the varying conditions of climate created by the greater land-forms are responded to by the various adjustments of minor land-forms and of plants, and how they are taken advantage of by man.

Planetary Conjunctions.

Some of our popular astronomical works of thirty or forty years back mention, among their lists of Mercury-transits across the Sun, one on November 4, 1901; and the conjunction of the planet and Sun that afternoon will have a narrow escape of a performance of such transit. A similar case occurred in 1638, when Horrox looked for one, but Mercury just escaped being projected on the Sun. The interval between A.D. 1638 and 1901 gives 263 years, which will be found, on experiment, to be a period of the recurrence of these transits. Newcomb, in his 'Astronomical Papers of the American Nautical Almanac,' vol. i. part vi., speaking of the conjunction of Mercury and the Sun on the morning of May 11, 1937, says:—"Mercury will pass so close to the Sun at inferior conjunction that it may almost be seen projected on the chromosphere. The nearest approach to the Sun's limb cannot be given without a more careful computation from the tables. It is, however, certain that it will be only a little more than a minute of arc." We cannot expect quite so close a conjunction as this next November.

Another conjunction next November is of more popular interest. On the 27th, at 18^h, we are informed under the head of "Phenomena," in the *Nautical Almanac*, ♃ ☌ ♄, ♃ 0° 27′ S., the planet Venus being not many degrees distant also. Although the planets are at a low altitude, and set only 1½ hours after the Sun, the conjunction will be a matter of some interest, as it can only be seen three or four times in a lifetime. The late Mr. Marth calculated the conjunctions of ♃ and ♄ for about 2000 years back and for about 1000 years to come, to see if there was an instance in which Jupiter might appear superposed on Saturn, as was the case with Venus and Mercury in 1737, but without success. In 'Scientiâ Eclipsium,' by Melchior a Briga, we are informed, "An. 1563 die 24 August. hor. 14 Junctinus vidit Saturnum australiorem in gr. 28 Cancri quasi cooperiri à Jove." This observation being made before the invention of the telescope, it is not likely that the objects were much closer together than will be the case next November. It is a matter of regret that we cannot obtain a conjunction of these planets similar to that of Jupiter and Venus on the morning of July 21st, 1859, when to the naked eye they came out of the clouds as a "double star of unusual composition." (See article on the said conjunction by Hon. Mrs. Ward, in *Recreative Science* for February 1860. The drawing appended, as taken with a 3-inch Dollond, shows Venus one diameter distant from Jupiter; but, being very far off at the time, it was small and almost as circular as the latter.)

Charles Leadbetter, in his treatise of Eclipses, published in the early part of the 18th century, gives some description of the conjunctions of ♃ and ♄ between the years 1702 and 1821. The results of computations from his imperfect tables may as well be given. He states:—"The first time of their meeting in Aries (since the year 968) is on Monday, May 11, 1702, 15^h 8^m longitude of them, Aries 6° 4′, ♄ lat. 2° 14′, ♃ lat. 1° 13′. Venus, Mercury, and Moon were very near them. The next conjunction was on December 27, 1722, at 8^h 4^m in 23° 41′ 10″ of Sagittarius. Saturn 1° 8′ 6″ N. lat. Jupiter's lat. 21′ 44″. Saturn elevated 46′ 22″ above Jupiter, and at the same time Mars is in Sagittarius 22° 39′ 56″ with 23′ S. lat. If the 28th day of December, 1722, be a clear morning, I desire all ingenious persons to cast their eyes on the S.E. horizon at 6.21. They will then see Saturn rising, and about a minute after, you will see Jupiter rise, and about 6 minutes after Jupiter you will see Mars rising: the like appearance has not happened in the memory of man." Mr. Leadbetter gives a diagram of the positions of these planets, and then favours his readers with a little astrology on the subject.

Referring to the third time of their meeting in the 18th century, we are informed that it is on Monday, August 23, 1742, 10^h 20^m in 27° 55′ of Leo. Saturn has 1° 17′ lat. N. Jupiter 52′ N. lat. "Here Saturn will be elevated above Jupiter (at the time of this conjunction) 25 min. They are then morning stars, and under the Sun's beams."

Continuing Leadbetter's predictions of the conjunctions of Jupiter and Saturn, with the results from his somewhat imperfect tables, we come to that of March 8, 1762 (O.S.). At 5.50 in the morning they were to meet in 12° 19′ of Aries. "Here Jupiter is elevated 107′ above Saturn. They are evening stars, 13° 40′ distant from the Sun, so cannot be seen."

Thursday, the 27th day of October, 1782, is given as the date of the next conjunction. At "15 min. past 5 o'clock in the morning they meet in 28° 22′ Sagittarius. Here Saturn will be elevated 46 min. above Jupiter; they are now evening stars, 43° 27′ distant from the Sun, so that in the evening after sunset they may be seen in the south-west, pleasant to behold."

The next conjunction of Saturn and Jupiter is in the year 1802. "It happeneth on Wednesday, July 9, at 18 min. past 11 o'clock at night, in 5 deg. 48 min. of Virgo. At the time of this conjunction Saturn will be elevated 37 min. above Jupiter; they are now evening stars, distant from the Sun 37 deg. 29 min., and consequently may be seen westward after sunset."

The next conjunction of these two planets is put down for June 6, 1821, at 8^h in the morning, "in 24 deg. 9 min. of Aries. They are both direct and swift in motion. Here Jupiter is elevated above Saturn 1° 9′: they are now morning stars, distant from the Sun 62 deg. 14 min., rising before him, pleasant to behold."

Looking at the distance that separated the planets at each conjunction described by Leadbetter, from 1702 to 1821, we must not despise the phenomenon of next November, for at the only one referred to when the objects were comparatively close, viz., that of 1742, they were lost in the Sun's beams. Francis Moore's Almanac for 1821 remarks:—"These two planets, even after their conjunction, seem unwilling to separate, the change in declination compensating for that in right ascension......On September 12 they will be nearly 5 degrees asunder; after that they will gradually approximate again, and at the end of the year, when they will be both evening stars, they will not be 2 degrees asunder." Partridge for that year adds that then "they will be on the meridian within 2 min. of each other, about half an hour past six o'clock in the evening." Moore gives a list of no less than eleven other conjunctions for 1821, but some of them would happen too near the Sun to be seen.

Subsequent conjunctions of Saturn and Jupiter took place on January 26, 1842, October 25, 1861, and April 22, 1881.

It may not be inopportune to bring the following forward now. At the bursting forth of the "Nova" of 1604, the brightest predecessor, for three centuries, of the "Nova Persei" we have recently been observing, Saturn and Jupiter were not far distant from each other, and Mars also near, and the new star burst forth near them also*. In the Paralipomena to Tycho Brahe's 'Historia Cœlestis' there is the following observation on the 27th of September at Prague, 13 days before the appearance of the stranger:—"Nondum incenso novo sidere pulcherrimum spectaculum vesperi fuit; quatuor planetæ, ♄, ♃, ♂, ☽ corniculata, eodem loco....Primus occidit ♂, deinde ☽, tum ♄, ultimus ♃, apparuitque, ad oculum jam tum transisse oppositionem ♂ et ♄." A diagram of the position of these objects is given, and then their places in degrees, &c., in the constellation Sagittarius.

It may here be mentioned that twice in the preceding century, and once in the following one, there were similar interesting appulses of the principal planets. "On February 11, 1524, Venus, Mars, Jupiter, and Saturn were very near each other, and Mercury not above 16 degrees from them; on November 11, 1544, Mercury, Venus, Jupiter, and Saturn were within the space of 10 degrees; also on March 17, 1725, Mercury, Venus, Mars, and Jupiter were so near each other as to be all seen through the same telescope without changing its position.

Melplash Vicarage, Bridport. S. J. Johnson.

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* The mention of the stars of 1604 and of Tycho's leads us to enquire what was the next-recorded temporary star previous to Tycho's. Humboldt gives a Chinese observation in 1230, elsewhere put down as a comet. But it is recorded that in 1388, "on March 29 a star appeared in the eastern part of the sidereal division of γ Pegasi." No mention of tail or motion seems recorded, and this is not in Williams's 'Comets observed in China.'

Note on the Period of ζ Geminorum.

Professor Campbell, in a valuable communication to the *Astrophysical Journal* for January, refers to my statement * that the period of ζ Geminorum lengthened to the extent of ten minutes between 1847 and 1869. He intimates, at the same time, that the alleged fact is unconfirmed by recent experience, citing Dr. Chandler as a witness to the star's punctuality during (say) the last decade. Now irregularities that vanish in the white light of modern research are open to the suspicion of never having existed at all; yet the conclusion should not be hastily adopted. In the present case I can only give my authority. It was Argelander †. He, to be sure, may have depended upon Schmidt's prior observations, although gratuitous assertions were by no means in his line. The matter is worth looking into at leisure.

The light-changes of the star were discovered by Schmidt in 1847; he found them nearly effaced in 1868, and capricious in 1881 ‡. Ascribing to them in 1872 § the identical period of $10^d\ 3^h\ 42^m\ 9^s$, to which they now conform, he remarked in it cyclical fluctuations self-compensatory at intervals of a few years. Since his death the phases are not known to have departed from a perfectly even course; but no very attentive watch has been kept upon them. It remains, therefore, an open question whether or no the Athenian astronomer was mistaken in his persuasion of their occasional disturbance.

The variation of ζ Geminorum, although small in range, is of a specially interesting kind. For, by a rare exception to the general rule, it proceeds symmetrically, the times of increase and decrease being, as nearly as possible, equal. It affects, moreover, a spectroscopic binary. The presence of a dark companion, revolving in the light-period, was detected by Bélopolsky in 1898, and independently by Campbell a year later. The latter has now computed the orbit of the pair, with curious results. His inference that eclipses do not occur might indeed have been anticipated from the examples of δ Cephei and of η Aquilæ; but the sinuous shape ascertained for the velocity-curve is a new and extraordinary feature. Superposed upon the smooth line representing the rise and fall of mean radial speed are a series of minor undulations, three of which fit precisely into the ten-day cycle, so that they have a period of 3·385 days. This commensurability precludes recourse to the otherwise tempting expedient of introducing a third component, since the system thus formed—as Professor Campbell points out—would almost necessarily be unstable. Nor is the tidal rationale, which he tentatively suggests, much more satisfactory.

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* 'System of the Stars,' p. 133.

† *Bonner Beob.* Bd. vii. p. 393 (1869).

‡ *Astr. Nach.* Nos. 1745, 2420.

§ *Astr. Nach.* No. 1880.

Zeta Geminorum gives a spectrum differing little from that of δ Cephei, which is of the solar type, with some reinforced lines. Its orbital movements tell nothing as to its mass; but indications of a different character warrant a conjecture on the subject. That the star is prodigiously remote can be gathered from the minuteness of its proper motion, estimated by Auwers at 0″·0165 annually. Now Professor Campbell deduces for the average radial velocity *in space* of 280 stars discussed by him a value of 17 kilometres per second[*]. That of ζ Geminorum, roughly corrected for the motion of the Sun, is 8 kilometres, or less than half the mean rate. The deficiency is presumably made up by the celerity of its tangential travelling. The average value of this component—again estimated by Professor Campbell for the same collection of stars—is just 27 kilometres. Allowing to ζ Geminorum no more than this velocity across the line of sight, we find that the parallax corresponding to the apparent proper motion is 0″·0027. At the indicated distance the Sun would be diminished to one-thirty-seventh the maximum brightness of the variable. And the two bodies being probably of almost equal density, their masses may be assumed proportionate to their light. We thus reach the startling conclusion that this rather inconspicuous star possesses about 225 times the solar gravitative power. The evidence to this effect should at least be taken into account in considering the structure and working of its system.

AGNES M. CLERKE.

Observations of the Poulkova Double Stars.

IN the latter part of 1898 I announced, in the 'Publications of the Astronomical Society of the Pacific,' that I had undertaken the re-measurement of the double stars discovered at Poulkova, and that 1350 observations of 414 of these stars had then been obtained. At that time 341 of the stars had been measured on three or four nights each, and were regarded as finished.

Nearly a year later I was very kindly asked to furnish a statement of the progress of this work for publication in *The Observatory*. Most of the measures had then been obtained, but so much remained to be done before the work would be in shape for printing that it seemed to me better to defer giving any account of it until it was ready. It has taken much longer to complete it than was expected, but I am glad to say that the manuscript is now ready, and it is intended to form a volume of the 'Publications of the Lick Observatory.' How soon it will be printed has not been determined, though it is hoped that it may not be long delayed.

In the beginning it was my intention to measure such of the

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[*] *Astrophysical Journal*, vol. xiii. p. 84.

Poulkova double stars only as are given in volume ix. of the 'Poulkova Observations.' Somewhat more than fifty years had elapsed since these stars were first measured by Otto Struve and Mädler, and about thirty years since the observations by Dembowski had been obtained. Many of the stars were known to be not particularly in need of measurement, but several considerations made it desirable that all should be embraced in my programme. It was thought, for example, that sets of recent measures would add materially to the data for the determination of orbits, proper motions, and systematic errors, and that they would also assist in pointing out the large number of stars in this list which have shown little or no motion and also those which are particularly in need of further attention.

As the work progressed it became apparent that to render it the most useful it should include all the double stars discovered at Poulkova. The programme was accordingly enlarged by the addition of all of the rejected stars. The work as originally outlined was expected to require about 1200 new observations. Many of the stars have been observed more frequently than was first contemplated. On this account, and also by reason of the inclusion of the rejected stars and the measurement of those companions subsequently discovered, the total number of observations has become 2171, or nearly a thousand in excess of the number first estimated. Of these about 1660 were made in 1898. In that year about 260 measures of miscellaneous stars were also secured.

The published measures of all observers have been collected, and the mean results of these are given, with references to the sources from which they were obtained. The orbits of several of the binaries have been computed, and the relative proper motions in a number of cases determined. The principal results of these investigations are given in the following tables.

Elements of Orbits of Binaries.

OΣ.	P.	T.	*e.*	*a.*	*i.*	☊.	λ.
	y			″	°	°	°
18...	182·75	1949·5	0·50	0·96	21·9	78·0	202·9
38...	55·0	1892·0	0·82	0·346	76·6	113·5	158·8
79...	200·0	1896·9	0·76	0·85	57·9	87·5	83·7
82...	97·94	1835·03	0·50	0·94	59·8	39·8	291·9
235...	66·0	1906·72	0·50	0·83	45·6	85·3	131·0
400...	78·67	1885·74	0·40	0·57	68·6	140·5	13·7
535...	5·70	1901·18	0·54	0·25	74·5	24·1	179·0

Co-ordinates for Rectilinear Motion.

OΣ.	$x = \rho \cos \theta.$	$y = \rho \sin \theta.$
47 A.B....	$+0''{\cdot}524 \quad +0''{\cdot}1143\,(t-1900)$	$-22''{\cdot}843 \quad -0''{\cdot}0783\,(t-1900)$
A.C....	$-28{\cdot}820 \quad +0{\cdot}1143\,(t-1900)$	$-15{\cdot}422 \quad -0{\cdot}0783\,(t-1900)$
A.D...	$-78{\cdot}436 \quad +0{\cdot}1143\,(t-1900)$	$-98{\cdot}580 \quad -0{\cdot}0783\,(t-1900)$
154	$-14{\cdot}356 \quad +0{\cdot}1502\,(t-1900)$	$+21{\cdot}979 \quad +0{\cdot}0223\,(t-1900)$
165	$+\ 3{\cdot}034 \quad +0{\cdot}1058\,(t-1900)$	$+\ 2{\cdot}622 \quad -0{\cdot}0066\,(t-1900)$
168 A.B....	$+\ 9{\cdot}84 \quad +0{\cdot}028\,(t-1900)$	$+22{\cdot}14 \quad +0{\cdot}038\,(t-1900)$
A.C....	$-21{\cdot}50 \quad +0{\cdot}028\,(t-1900)$	$+47{\cdot}38 \quad +0{\cdot}038\,(t-1900)$
223	$-15{\cdot}62 \quad +0{\cdot}000\,(t-1900)$	$+14{\cdot}60 \quad +0{\cdot}132\,(t-1900)$
297	$-\ 3{\cdot}832 \quad +0{\cdot}1370\,(t-1900)$	$+\ 3{\cdot}245 \quad -0{\cdot}0722\,(t-1900)$
311	$-\ 6{\cdot}269 \quad +0{\cdot}1336\,(t-1900)$	$-\ 2{\cdot}587 \quad -0{\cdot}0354\,(t-1900)$
317 A.B....	$-13{\cdot}30 \quad -0{\cdot}080\,(t-1900)$	$-11{\cdot}80 \quad +0{\cdot}025\,(t-1900)$
A.C....	$+85{\cdot}97 \quad +0{\cdot}047\,(t-1900)$	$-78{\cdot}57 \quad -0{\cdot}134\,(t-1900)$
413	$+\ 0{\cdot}2958 \quad +0{\cdot}0096\,(t-1900)$	$+\ 0{\cdot}6475 \quad +0{\cdot}0002\,(t-1900)$
477	$-\ 4{\cdot}286 \quad +0{\cdot}0134\,(t-1900)$	$-\ 1{\cdot}403 \quad -0{\cdot}1764\,(t-1900)$
533	$+\ 9{\cdot}143 \quad -0{\cdot}0107\,(t-1900)$	$-12{\cdot}481 \quad -0{\cdot}3059\,(t-1900)$

The above orbits have been derived by various analytical and graphical methods, the method used for a given star depending largely upon the amount and character of the data available for the discussion. The co-ordinates for rectilinear motion are the results obtained by making the solution according to the method of least squares, subject to the condition that the velocity of the companion in each case is uniform. It will be seen that OΣ 413, λ Cygni, is here included among the proper motion stars. In accordance with the coefficients of the variable terms of the co-ordinates of this pair, as given above, the relative proper motion of the principal star is

$$\mu = -0^{s}{\cdot}00002, \quad \mu' = -0''{\cdot}0096.$$

According to Newcomb's 'Catalogue of Fundamental Stars' the proper motion of λ Cygni is

$$\mu = +0^{s}{\cdot}00088, \quad \mu' = -0''{\cdot}0057.$$

In comparing these values it should be remembered that this is a close pair, not resolvable with meridian circles. Consequently, the proper motion derived from the meridian observations corresponds to what we may call the centre of gravity of the light of the two stars. This is less than the total motion, in this case not much more than half of it. In view of this consideration the results afforded by the meridian observations and the micrometrical measures are in substantial agreement. This renders it very probable that this pair, which has commonly been regarded as a binary, and for which a system of elements has been computed, is an optically double star only.

Lick Observatory, **W. J. Hussey.**
1901, Feb. 12.

The Variability of Eros.

THIS variability was announced last month, and the announcement at first was received with caution and hesitation. But so many observers have now directed their attention to the question, with satisfactorily accordant results, that there can be little, if any, doubt of its reality. The period was at first approximately announced as $\frac{1}{10}$ or $\frac{1}{9}$ of a day, and finally more accurately fixed at $2^h\ 37^m{\cdot}6$. M. Ch. André, however, from a discussion of the Lyons observations, concluded that the successive waves of the light variation were not precisely similar, and that the *complete* light variation took place in double the above time, or $5^h\ 16^m$. He subsequently examined the observations made by Herr Deichmuller at Bonn, and found that they, too, favoured this double period, which he therefore considers as established. The amount of variation seems to be at least a magnitude; many observers make it 1½ or even 2 magnitudes, which implies that the light at maximum is three times that at minimum, a most startling difference. Starting from the first minimum, M. André gives the times of the different phases as follows :—

	Bonn.		Lyons.	
	h	m	h	m
First minimum	0	0	0	0
First maximum	1	16	1	20
Second minimum	2	47	2	51
Second maximum	4	2	4	10
Third minimum	5	10	5	16

These agree in making the second wave shorter than the first by about 25 minutes; this is, however, not an impossible error in observations of this character, though it is improbable.

The light-curve, as mapped by M. André, is almost precisely similar to those of β Lyræ and U Pegasi; the variation of light is continuous without any flat portion like stars of the Algol type. The variation is, however, much less rapid near maximum than near minimum. The first and the second waves are nearly similar, but the second wave is slightly more contracted, as the above figures show.

It would appear that the variation is larger than can reasonably be attributed to rotation alone, and M. André has recourse to the hypothesis that the planet is double, and that the plane of revolution passed through the Earth in February. The complete period of revolution is $5^h\ 16^m$, and the two minima are explained by each component in turn occulting the other. The inequality between the waves is explained by supposing that the relative orbit is elliptical. And since the variation is larger than the mutual occultations alone would explain, he further supposes that each component is greatly elongated in the direction of the line of centres, the section at right angles to this line being nearly circular.

The two components are supposed to be very nearly in contact. Thus at a maximum the appearance would be like that of a pair of spectacles with elongated lenses, while at a minimum only a single circular outline would be presented.

The following are the elements of the system as given by M. André (*Ast. Nach.* 3698):—

Period of revolution, $5^h\ 16^m$.
Eccentricity of relative orbit, 0·057.
Angle from line of nodes to periastron, 162°·4.
Semi-axis major of relative orbit only slightly greater than sum of radii.

The two components are nearly equal, the ratio of their linear dimensions not exceeding 3 to 2.

The mean density of the system is 2·4.

The sections of the components by planes passing through the line of centres are ellipses whose axes are in the ratio of 2 to 1.

It is scarcely necessary to say that this hypothesis is only given provisionally, and as affording a possible explanation of the light-curve. The idea of a double minor planet is an utterly unexpected one, and one that does not at first sight commend itself as at all probable. But if we once admit the initial fact that Eros in February was subject to variations of light considerably exceeding a magnitude—a fact for which the evidence is certainly strong,—we seem almost driven to some such hypothesis as that of M. André.

It is clear that if Eros is double, its variations will not always follow the same law. For the plane of revolution will only occasionally pass through the Earth. That part of the variation, however, which depended on the elliptical shape of the components would still persist, even when mutual occultation no longer took place. One would have expected that the duplicity if real would have been detected with the Yerkes and Lick telescopes, both of which have been directed on the planet. The separation, however, would probably be only a small fraction of a second and so might be missed. The immense stock of photographs that were taken, both in 1898 to 1899 and at the present opposition, should make it possible to test M. Andre's hypothesis.

Comptes Rendus of March 11 contains a description of the photographic observations of variability made at Toulouse by M. L. Montangerand. The planet was allowed to trail on the plate and the maxima and minima could thus be located. The period from the mean of photographic and visual observations is given as $2^h\ 38^m$, which is exactly half the $5^h\ 16^m$ of M. André.

On February 23, 25, 27 the range from maximum to minimum was $1^m{\cdot}5$. A photograph on March 8 showed that the range of magnitude was less than on February 23. This would be in accordance with M. André's hypothesis, if we suppose that by the latter date the mutual occultations were only partial. The whole investigation is a decidedly novel one, and lends still further interest and romance to the already unique planet Eros.

A. C. D. Crommelin.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Annular Solar Eclipse of October 7, A.D. 218.

Gentlemen,—

There are some special circumstances connected with the historical record of this eclipse, on which account I beg to be allowed space for a few words with reference to it. In the first place, the historian who speaks of it was probably an eye-witness and a trustworthy narrator. But unfortunately the later books of Dion Cassius are lost, and we have only their abridgment by the feeble writer Xiphilin of Trebizond (nephew of the patriarch of Constantinople of the same name), who lived more than eight centuries afterwards, or about the time of the Norman Conquest in England. As we have the account (lib. lxxviii. c. 30), towards the end of the short reign of Opilius Macrinus, whilst the rebellion was preparing which led to his displacement by Elagabalus, a prodigy, we are told, took place, if ever there was one; and then two celestial appearances are mentioned, a remarkable eclipse of the Sun—*ἡλιου τε γαρ ἔκλειψις περιφανεστάτη ὑπὸ τὰς ἡηέρας ἐκείνας ἐγένετο*—and a comet-star which stretched its tail for many nights from the west towards the east, and frightened the spectators so much, says the historian, that they could not help thinking of a passage in the twenty-first book of the Iliad, when, whilst Achilles is driving the Trojans into the city, the gods take part in the contest and the whole heavens gave forth as it were a trumpet sound. Now this event (I don't mean the battle at the river Scamander, but the sedition raised by the Syrian army against Macrinus) took place early in the year A.D. 218, and the battle in which that pusillanimous emperor was defeated, after a reign of about fourteen months, was fought on June 8 in the same year. Dion held office under Macrinus, as he afterwards did under Alexander Severus, in whose reign he probably died.

Now Dr. Ginzel informs us, in his monumental work 'Spezieller Kanon der Sonnen- und Mondfinsternisse . . . . von 900 vor Chr. bis 600 nach Chr.,' that no solar eclipse answers the conditions in the whole of the third century except the annular one of October 7, A.D. 218, the central line of which passed over part of Asia Minor and Syria. He suggests, therefore, that either Dion Cassius had forgotten the time of the year when the eclipse took place (his expression, it will be noticed, is very indefinite) or that Xiphilin had confused and misplaced part of his narrative. Dion would seem to have been governor of part of the province of Asia at the time in question.

As regards the comet, it is well known that Hind identified it with a return of Halley's Comet, which would be due in perihelion in the spring of A.D. 218. Yours faithfully,

Blackheath, 1901, Mar. 15. W. T. Lynn.

The New Star of 1572.

Gentlemen,—

In the article on "Temporary Stars," in your March number (p. 126), you say that "Tycho's star, in 1572, was first seen as bright as Jupiter and increased quickly, so that it became equal to Venus." Allow me to point out that the star, when first seen by Tycho, was brighter than Jupiter at minimum distance and was as nearly as possible equal to Venus at its maximum brightness. The few observers who saw it some days earlier than Tycho did not say anything about its magnitude.

The erroneous statement that the star was at first only equal to Jupiter, appears in a few modern books (even in Young's 'General Astronomy') and has probably originally arisen through careless reading of Tycho's own words. As some hypothesis or theory about the nature of these bodies may some day be founded on the alleged slow increase of Tycho's star, it may be well to point out the mistake. I have given a full review of the literature on this star in my 'Tycho Brahe,' chapters 3 and 8.

Whether really "Tycho's star remains in the heavens, a shadow of its former greatness," is somewhat uncertain. No doubt there is a small star within a minute of arc of the most probable place of the Nova, but it is obvious that we cannot be sure of the identity of the two. Yours faithfully,

J. L. E. Dreyer.

[We are obliged to Dr. Dreyer for his accurate information as to the brightness of the star when first seen by Tycho. Dr. Dreyer in his book says that Argelander, from a discussion of Tycho's distance-measures, deduced a position of the star which agrees remarkably well with that of a star of magnitude 10·11 in d'Arrest's list of stars in the neighbourhood of the star of 1572. It has been suggested that Tycho's star is a variable with period of 300 years; in 1872–73, therefore, Dr. Hind and Mr. W. E. Plummer made observations of d'Arrest's star, and these seemed to indicate that it is subject to slight fluctuations of brightness. In Chandler's '3rd Catalogue of Variable Stars,' the star of 1572 is called B Cassiopeiæ; by a typographical error this was called β on p. 127 of our last number.—Eds.]

The Firm of Nairne and Blunt.

Gentlemen,—

This Observatory has an old reflecting telescope of the Newtonian pattern, made by Nairne and Blunt, of London. I have been requested to obtain information in regard to this firm, but so far have been unable to get anything. Mr. Wesley, Assistant Secretary of the Royal Astronomical Society, has given me some information in regard to Edward Nairne, but I have not been able to learn anything about the firm. I would be very glad if your readers could furnish me any information.

Yours respectfully,
J. K. Rees,
Director of the Observatory.

Columbia University of New York,
1901, March 14.

NOTES.

Comet Notes.—Prof. Kreutz has brought out a very interesting volume on the family of Comets 1843 I., 1880 I., 1882 II., &c. Members of this family are remarkable for their brilliance, their small heads and long straight tails, their extremely small perihelion distances, and the close resemblance, though not absolute identity, of their orbit elements. The most probable period of 1843 I. is 512 years, but a period of 800 years (viz. the same as that of 1882 II.) is quite possible. The observations do not admit of a period of 37, 39, or 175 years, so that the hypothesis of identity with 1668, 1880 I., or 1882 II. is untenable; on the other hand, the assumption of parabolic motion is equally inadmissible.

The Comet 1880 I. was at first identified with that of 1843, but after the appearance of 1882 II. this was definitely disproved.

The Comet 1887 I. was only visible for about a week, and having no definite nucleus was an exceedingly difficult object to observe. It is not possible to deduce very exact elements, but the comet evidently belongs to the same system as the others.

Although the orbit of the comet of 1680 does not very closely resemble those of this family, it is thought probable that it and 1882 II. are fragments of a single comet which suffered disruption when near the Sun. But as the period of the 1680 Comet cannot well be less than 2000 years, and is probably 8000, the disruption must be very ancient.

The suggestion that the eclipse comet of 1882 May 16 may have belonged to this family is examined and shown to be very plausible. The observed position would admit of an orbit similar to that of 1843 I., and the comet, if moving in such an orbit, would have remained for a long time involved in the rays of the Sun, which would explain M. Trépied's failure to detect it after the eclipse, in spite of repeated searches both in the morning and evening.

A similar investigation was made for the eclipse comet of 1893 April 16; but it was found that although the comet had probably a very small perihelion distance, its observed motion precluded the possibility of its belonging to this family.

Pogson's Comet of 1872 December 2 is next discussed, and it is shown that the resemblance of its orbit to those of the family we are concerned with is not improbable. But as the comet was only observed on two days, the matter must remain in doubt.

Several comets of the 16th and 17th centuries which *may* have belonged to the family are dealt with, but the observations are not precise enough to admit of a definite answer. This is still more the case with a great number of old comets which are enumerated, but in which there is little beyond physical resemblance to lead one to conjecture that they may be members of the family.

The most probable elements of these comets are collected here for convenience of comparison. The times of perihelion passage are all expressed in Berlin M.T., and the Equator and Equinox to which each orbit is referred are given, also the period in years.

Comet ...	1680.	1843 I.	1880 I.	1882 II.	1887 I.
T	1680 Dec. 18^d·0248	1843 Feb. 27^d·44820	1880 Jan. 27^d·63370	1882 Sept.	1887 Jan. 11^d·16865
ω	350° 37′ 48″	82° 38′ 2″	85° 6′ 3″	69° 35′ 16″	58° 21′ 6″
☊	274 57 48	1 19 51	4 45 25	345 58 58	324 37 42
i	60 40 36	144 20 4	144 30 41	141 59 43	128 27 54
log *q*	7·79396	7·74251	7·74390	7·89004	7·98521
e............	0·99998	0·99991	0·99994	0·99991	Parabolic.
a	414·83	64·03	86·18	86·18	
Period ...	8449	511	800	800	
Equinox.	1880·0	1843·0	1880·0	1880·0	1887·0

A. C. D. C.

Minor Planet Notes.—A new planet, GH, of magnitude 11·5, was discovered by Prof. Wolf on March 13. It may be identical with (449) *.

The following planets, both discovered by him, have received names:—(442) Eichsfeldia, (457) Alleghenia. He notes that the latter was the first planet discovered with his new lens, and the name is a compliment to Mr. Brashear, of Allegheny, the maker of the lens.

The Eros parallax campaign may be regarded as at an end. Some more photographs may be taken, but these will not be for parallax, but merely for better determination of the orbit. No. 77 of the 'Publications of the Astronomical Society of the Pacific' contains a summary of the work that has been done at the Lick Observatory. Messrs. Hussey and Aitken have made visual observations with the 36-inch on 26 nights between Oct. 21 and Jan. 1. The observations are pretty well distributed on the two sides of the meridian.

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* This identity has since been established.

Messrs. Perrine and Palmer have taken over 800 plates of Eros with the Crossley reflector. The best of these will be selected and measured by Prof. Rees, Director of the Columbia University Observatory. Some plates taken last September have already been measured, and the results are very promising.

Messrs. Tucker and Crawford have observed on the meridian the whole of the stars on both the lists issued by the Paris Conference, each star receiving at least two observations. The resulting mean places of the stars on the first list have already been printed and circulated, which is a remarkably smart piece of work. A. C. D. C.

NOVA PERSEI.—The facts of observation relating to this wonderful object between February 25 and March 7 will be found in our Report of the Meeting of the Astronomical Society. It is there stated that its magnitude on March 7 was about 3. Since that date, speaking generally, the Nova has slowly but gradually diminished in brightness, until on March 25 it was of about the same magnitude or possibly fainter than *l* Persei, which is given in the Harvard Photometry as 4·8. In a brief period of clear sky on March 26 it appeared to observers at Greenwich to have considerably brightened since the previous night and was possibly as bright as a fourth-magnitude star, and there is some evidence of fluctuation on other dates. In colour the star has grown more ruddy, and on March 25 and 26 was seen in the telescope of a deep red colour.

Several observers have published short notes (many in the *English Mechanic*) of their observations of the spectrum, agreeing in the main fact that on February 28 and in the early days of March the spectrum was continuous with bright hydrogen lines. Mr. Espin says that on March 6 the "F & C hydrogen lines were very broad and also the third hydrogen line. The nebula line between the 3rd and F was obvious." But the most interesting information, because it relates to the days before February 25, when no observations were being made on this side of the Atlantic, comes from Prof. Pickering, who devotes Harvard Circular No. 56 to the subject. He says that fortunately for some weeks past an attempt has been made to photograph the whole sky both east and west of the meridian at short intervals, a Cooke lens and a Ross-Zeiss Anastigmat lens being used for the purpose, and in consequence photographs of the region of the Nova taken on February 2, 6, 8, 18, and 19 were available. On receipt of the announcement of Dr. Anderson's discovery on February 22 these were at once examined, but though they showed stars as faint as the 11th magnitude, no trace of the Nova was seen. The evening of the 22nd was cloudy, but, through breaks in the clouds, eye comparisons with other stars showed that its magnitude was about 0·9, and photometric determinations at

$14^h\ 0^m$ and $17^h\ 25^m$ G.M.T. gave the magnitudes 0·35 and 0·39 respectively. The circular goes on:—

"On this same evening, February 22, 18 photographs were taken with various instruments, under the direction of Mr. Edward S. King. They showed that, photographically, the Nova was 0·3 fainter than α Aurigæ. The general appearance of the photographic spectrum resembled that of the Orion type and was very unlike that of other new stars, in which the bright lines are the most conspicuous feature. This star had a strong continuous spectrum traversed by 33 dark lines. The approximate wave-lengths, as derived by Hartmann's formula, from the measures of Hε, Hγ, and Hβ, are given below. Each is followed by its relative intensity, and by the difference found by subtracting it from the wave-length of the corresponding line, if any, in the spectrum of β Orionis. As the lines having greater wave-length than 5000 have thus been determined by extrapolation, they may be subject to large systematic errors.

"3894, 10, Hζ, −5; 3970, 20, Hε, 0; 4026, 3, 0; 4077, 2, −1; 4102, 30, Hδ, 0; 4126, 5, +2; 4151, 1, −4; 4266, 2, +1; 4341, 40, Hγ, 0; 4366, 1, +1; 4388, 2, 0; 4415, 1; 4435, 1, +3; 4470, 2, +2; 4481, 20, 0; 4510, 2, −2; 4530, 2; 4552, 2; 4572, 1; 4616, 1; 4643, 1; 4665, 3; 4714, 3, −1; 4862, 40, Hβ, 0; 4885, 2; 4922, 2, 0; 5325, 1; 5399, 1; 5431, 1; 5677, 2; 5695, 7; 5719, 5; and 5761, 1. On careful examination the lines 3970, 4102, 4341, 4481, and 4862 were seen to be bright on the edge of greater wave-length. The line 4665 was bright on the edge of shorter wave-length, or there was a bright line whose approximate wave-length was 4660. The line 4026 was not measured, but identified from its position.

"On February 23 the clouds were so dense that few observations could be made. The star appeared to be brighter and bluer than α Aurigæ and to have the approximate magnitude 0·0. The spectrum was photographed faintly and showed no marked change except that the line K, which was absent on the previous evening, was present and nearly as intense as Hε.

"On February 24 it became clear soon after noon, and at one o'clock the Nova was seen with the 6-inch Equatorial, and also with the 2-inch finder, in strong sunlight. In the evening the magnitude, according to visual comparisons, was 0·54, from measures with the 15-inch Equatorial 0·59, and with the meridian photometer, in strong daylight, 0·28. Photographically it was 0·4 or 0·5 fainter than α Aurigæ. The spectrum showed a remarkable change. It was traversed by numerous bright and dark bands, and closely resembled that of Nova Aurigæ. The principal lines were dark with accompanying bright lines of somewhat greater wave-length. The bright lines accompanying K and Hε were reversed, and traversed by narrow well-defined dark lines. These last lines, and one of somewhat shorter wave-length than Hβ, are the only sharply defined lines in the spectrum, all of the others being broad and hazy, and difficult to measure with accuracy.

"Clouds interfered with observations on February 25, but the Nova was evidently much fainter than on the previous evening. Its magnitude from visual comparisons was 1·4, from photometric measures 1·07. The spectrum differed slightly from that on February 24. The lines Hδ, Hγ, and Hβ were also reversed and replaced by one or more narrow dark lines.

"On February 26 the magnitude from visual comparisons was 1·3, from photometric measures 1·49. The changes in the spectrum were slight.

"It therefore appears that on and before February 19, 1901, the star was invisible, or at least fainter than the eleventh magnitude. On February 21 its magnitude was 2·7, according to Dr. Anderson. On February 22 its magnitude was 0·5, perhaps becoming a little brighter on February 23, and then diminishing, so that on February 25 its magnitude was 1·1. Its spectrum on February 22 and 23 was of the Orion type, nearly continuous, traversed by narrow dark lines. During the next 24 hours an extraordinary change took place, so that on February 24 the spectrum resembled that of the other Novæ. It was traversed by bright and dark bands, and the principal dark lines had accompanying bright lines of slightly greater wave-length."

M. Deslandres has published two notes in the *Comptes Rendus* of the observations made at Meudon. His description of the spectrum on February 26 and 28 is very similar to that given by others as to the broad bright hydrogen lines, with their centres displaced towards the red, and accompanied each by a dark band on the violet side. In particular he says the Hβ band had three maxima of light, or, as it may be otherwise described, the bright band appears to be crossed by two dark diffuse lines. In his second note he says that on March 5 there are three such dark lines, all well defined; on March 6 he could distinguish four dark lines; and on March 8 these lines were much finer and had the appearance of reversed lines. He adds:—

"The broad dark band which is adjoined on the violet side to the bright band is only shown sharply on the plates of rather long exposure. Its displacement towards the violet corresponds to a velocity of approach of hundreds of kilometres per second; but the measure of this displacement has not been given because it is uncertain on account of the breadth of the band and the want of definition of its edges. Besides it may not correspond to the actual velocity, for the dark band may be partly masked by the bright. The spectrophotograph on March 12 shows in the large dark band three brilliant lines, quite sharp, and a fourth only suspected. The displacements of these bright lines are considerable, and are equivalent to velocities of approximately −1200 km., −1600 km., and −1850 km. per second."

A Kiel circular, received just as we go to press, gives a suggestion, made independently by Prof. Glasenapp and Prof. Duner, that the Nova is variable, with a period of 3 days, minima having happened on March 19, 22, and 25.

THE COMING ECLIPSE.—An expedition, small in number owing to the reduced size of the observing-staff and the pressure of work on the *Eros* campaign, will be sent from the Lick Observatory at the expense of the generous Mr. Crocker to observe the Solar Eclipse. The expedition left San Francisco on February 19, and, travelling *viâ* Yokohama, Hong Kong, and Singapore, hoped to reach Padang, Sumatra, about April 10. Mr. Perrine is in charge, accompanied by one assistant, and he proposed to secure the services of ten or twelve local assistants. The programme is limited to investigations of the corona, and to a search for unknown objects in the vicinity of the Sun, such as comets or unknown planets. A spectroscopic observer could not be spared from Lick, but two simple spectrographs will be taken, in the hope that they may be made use of.

Prof. D. P. Todd writes to us:—"Quite unexpectedly I am starting for the eclipse, hoping to reach Singapore by the middle of April. Probably I shall locate on the island of Linga or Singkep, 170 miles S.E. of Singapore. My main instrument will be a 12-inch mirror of speculum metal fitted with a revolving occulter, in the expectation of getting a few coronal streamers on a single plate and rightly timed throughout their whole length. A botanist and perhaps an anthropologist will accompany my expedition, and Mrs. Todd goes also to get material for her book on Total Eclipses."

Mr. Dyson, on his way to Sumatra with Mr. Newall, writes to us from Latitude $+33^\circ 43'$, Longitude $1^h 45^m 8^s$ East, under date March 18. He says that they have made the acquaintance of a large party of Dutch astronomers—Dr. Julius, Dr. Nijland, Mr. J. H. Wilterdinck, and Mr. Hudebrecht—who are on board the same vessel and have kindly given him details of their comprehensive programme. They purpose taking scale photographs of the eclipsed Sun with a 4-inch lens of 40 feet focal length, and to use short-focus lenses for Burckhalter's method. They also have a prismatic camera (Cooke Triple, 4-inch aperture, 7 feet focal length, and two prisms of 45° each); two slit spectroscopes, one of which will be used by Mr. Wilterdinck to determine the rotation of the corona; and a grating of 14,000 lines to the inch. They also intend to make polariscope observations and determinations of heat radiation by means of an Angström's pyrheliometer and thermopile, and also observations of temperature, earth magnetism, and other things.

Another party on the same ship hails from the Massachusetts Institute of Technology. Their programme is not so extensive as that of the Dutch astronomers. They propose to make magnetic observations to detect any possible effect of the cooling produced by the phenomenon, and will also take the opportunity to make some gravity experiments in Sumatra.

So far, then, as we know at present, Sumatra will be occupied by the British observing party, the Dutch expedition as above described, the party from the Calcutta Jesuit Mission mentioned in our February number, and the American representatives—Prof. Barnard from Yerkes, Mr. Perrine from Lick, Mr. Abbott from the Smithsonian Institution, Prof. Brown of Washington, and perhaps Mr. Burckhalter, but of this last we are not sure, whilst Prof. Todd will be comparatively near at hand.

A DETERMINATION OF THE SOLAR MOTION.—In the early days of the determination by spectroscopy of the motion of stars in the line of sight, it was suggested that such results could be made to give a value of the motion of our system in space. We believe that the first attempt at such a research was made by M. Homann, who published, in *Ast. Nach.* No. 2714, a result based on the spectroscopic observations of 49 stars; the most recent (there have probably been others between these) is by Prof. W. W. Campbell, who publishes an article on the subject in the *Astrophysical Journal* for January. The principle of the method may be enunciated thus:—On the whole the stars which lie in front of the Sun, looking in the direction in which he is going, will have an apparent motion of approach, those which lie behind the Sun will, on the whole, apparently recede. Prof. Campbell puts this in rather a different way: from determinations of the velocities of 280 stars, he finds the velocity of the solar system with reference to the mean of them, treating the actual motions of the individual stars as though they were accidental errors. The exact procedure is as follows:—If V be the motion of the solar system in space, v be the observed velocity of a star, D be the angle between the solar apex and this star, then $V \cos D - v$ is considered $= 0$, so that the velocity of the star relative to the mean of the 280 is considered a quantity which will appear as a residual in the solution; D is obviously a function of the position of the star and of the solar apex which is easily evaluated, and a solution of the 80 equations (the 280 stars are collected into 80 groups) gives a value of V (19·89 km. per second, with a probable error of 1·52 km.) and a position of the apex of the Sun's way:

$$\alpha = 277^\circ\ 30' \pm 4^\circ\!\cdot\!8, \quad \delta = +19^\circ\ 58' \pm 5^\circ\!\cdot\!9.$$

THE REGIONS OF SOLAR ACTIVITY.—Just as spots appear in particular solar latitudes at certain phases of the spot-cycle, so, it has often been suggested, there should be some analogous law which regulates the longitudes of regions of activity in the Sun. M. Wolfer, of the Zurich Observatory, has published a research on this subject in a recent number of the 'Journal of the Italian Spectroscopic Society' (vol. xxix. part 10). The author tabulates the positions of the groups of faculæ in the years 1887 to 1892, and finds that in the years 1887 to 1889 they congregated about certain centres, the longitudes of which increased, but that in the years 1890 to 1892, a sun-spot minimum having happened in 1889·6, the faculæ grouped about centres whose longitude diminished; but in computing these longitudes, Sporer's mean value of the daily rotation had been used, and in the first period the mean latitude of the faculæ was +5°, but in the latter period was +22°, so that this apparent change of longitude is proof of the fact that Sporer's law as to the change of rate of rotation with latitude holds for faculæ as well as for spots, in fact the figures deduced from these faculæ agree almost exactly with those of Sporer's law. The main conclusion of M. Wolfer's paper appears to be that at the expiration of a sun-spot period the faculæ collect near the equator of the Sun in certain longitudes, and the first faculæ of the new period arise in *the same longitudes* but in high latitudes.

THE LEONID METEORS.—There is a Report, which comes *via* the Toronto Astronomical Society, that an observer at York Factory, Hudson's Bay, saw a considerable meteor-shower on November 15 and 16 last. The notes are as follows:—"November 15, 1900. Very general display of shooting-stars. Some very big ones N.W. to S.E. Sky full in shoals.—November 16. Shooting-stars seen until daylight. Scared the people: they thought it was the end of the world."

THE MAKING OF A LENS.—Messrs. Carl Zeiss have been kind enough to send us a reprint of an article from McClure's Magazine for October last (it is also in the *Idler* for March), which describes and includes graphic illustrations of the great glass-works at Jena. There is, among other things, the account of the casting of a lens, which is interesting enough to reproduce. After describing the pouring of the molten glass from the crucible into the mould, which was more than 4 feet diameter, the description goes on:—

> The mold, with the glowing lens inside, was now covered with a plate of iron, wheeled to the mouth of the cooling furnace, and lifted with chain tackle to the height of the furnace floor. A movable-frame tramway was then placed underneath it, and it was quickly pushed into the furnace. Workmen were ready with brick and mortar, and in ten minutes the lens was walled in. Here it is cooled for two weeks, and then brought again to the open air, dull and milky of surface and possessing only the general shape of a lens. After that, for days and weeks, workmen are employed in polishing it, not to give it the final form which it will have in the great telescope, but merely to prepare it for that important and anxious day when it will be submitted to those searching tests for imperfections, during which it must pass even the close scrutiny of microscopic and spectroscopic examination. A few bubbles it may have and pass, for bubbles have no effect, except to reduce the passage of light in a minute degree; but veins, denoting the improper mixture of the ingredients of the glass, it must not have. If it passes all the tests—and sometimes it requires many castings and costs many rejected lenses of this most precious of glass before the necessary perfection is attained—it is again sent to the furnace house, where with even greater care than before it is slowly raised to a high temperature, and thus annealed, and then as slowly cooled for two months or more. After that it is ready for the lens-maker proper, that skilled mechanician and mathematician of Jena or of

America or of France, who polishes down its sides with infinite care, until they reach the most perfect curves appropriate to the refraction and dispersion of the glass disks employed. Each of these processes has absorbed precious time and has cost much money; the bare glass for such a lens would cost about $5,000. To this the skill of the optician would add in polishing perhaps $20,000 more, so that the finished lens, ready for fitting into the telescope tube, would represent an expenditure of some $25,000. Through such pains and expense as this must science pass that mankind may add a few facts to its knowledge of some distant star.

A CATALOGUE OF VARIABLE STARS.—Mr. A. W. Roberts, of Lovedale, South Africa, has published, in *Astronomical Journal*, Nos. 491–492, the elements of 93 variable stars south of declination 30°, deduced from his own observations, and from those of other observers which have been placed at his disposal. It is scarcely necessary to say more beyond that the catalogue is in the form made familiar to us by Dr. Chandler's similar work, for those interested in the subject will no doubt obtain the copies of the *A. J.*; but the following paragraphs from Mr. Roberts's introduction are of sufficient general interest to quote:—

> The great increase in brightness of some of the long-period stars is indeed remarkable. For instance the star (5096) *R Centauri* is at least three hundred times brighter at a maximum than it is at a minimum. It is difficult to conceive how such a stupendous increase in light and heat should not end in the complete disintegration of the star.
>
> It has sometimes been stated that all long-period variables are red in colour, and short-period variables are white. On this matter I am not able to speak with certainty or with confidence, as my own appreciation of colour is poor, arising either from actual inability to readily distinguish different shades of colour, or from want of experience.
>
> It is evident, of course, from a consideration of the material supplied by many observers, that a majority of long-period variables are red, and that a majority of short-period variables are not red; but that all short-period stars are white I am inclined to doubt.

THE WEATHER IN MARCH.—Mild humid weather prevailed in the first week of March, in continuation of the weather which had characterized the latter part of February. The prevalent wind was S.W.; more or less rain fell every day to the total amount of one inch, and the excess of mean temperature as compared with the average was 3°. The highest temperature recorded was 54°·1 on the 5th. A prolonged period of March weather ensued until the 29th, with bleak north-easterly winds and occasional cold showers of rain or snow, producing a measured amount of 0·55 inch in seven days. After the 25th the weather became intensely cold, the mean daily temperatures being 10° to 12° below the average values, and the night temperatures falling 6° to 8° below freezing-point. The mean temperature for the 22 days, March 8 to 29 inclusive, was 37°·1, being 4½° below the average. The month was generally cloudy, and the amount of sunshine was small. W. C. N.

MR. LYNN's books are too well known to our readers to render any description of them necessary; but Messrs. Sampson Low, Marston, & Co., who are now Mr. Lynn's publishers, have favoured us with a copy of the tenth edition of 'Celestial Motions,' which is getting to be quite a thick book, as it grows year by year. It is unfortunate that Nova Persei was not discovered a little earlier, as it would, no doubt, have found a place on p. 71. From the same firm we have received new editions of 'Remarkable Comets' and 'Remarkable Eclipses,' which, as we have before said, are each most excellent value for a sixpence.

ERRATA IN THE 'COMPANION,' 1901.—Page 33, June 10, *for* 334 *read* 335; June 26, *for* 4 *read* 341; August 5, *for* 290 *read* 265.

IT is worth while putting on record that the Meeting of the Astronomical Society in March, which was the first held under the new arrangement of hour, was fully attended, the seats being filled even to the back row. This may have been due to the attraction of the papers on the discovery of the new star as much as to the change in the hour of meeting.

MR. A. F. LINDEMANN, of Sidmouth, England, has supplied a fund to the Astronomische Gesellschaft to furnish rewards to anyone who will help cometary research by computing the orbit of a comet. In *Astronomical Journal*, No. 493, a list of comets from 1757 to 1852 whose orbits have not yet been computed is given, from which intending computers can select.

THE Lick Observatory has begun the issue of a series of Bulletins, to announce the more important discoveries and results of observation whose usefulness depends on prompt publication. The first Bulletin gives the results of meridian-circle observations of reference stars for measurement of Eros at opposition.

THE next Meeting of the Royal Astronomical Society will be on Friday, April 12; of the British Astronomical Association on April 24.

FROM AN OXFORD NOTE-BOOK.

A GIGANTIC project in the department of meridian astronomy is announced from Berlin by Dr. Auwers: no less than a collection, collation, and publication of all the meridian observations between 1750–1900, as a huge catalogue, with all the information for proper motions. Germany has shown what she can do in this way by organizing the work for the Catalogue of the Astronomische Gesellschaft; and no doubt she will carry this new scheme through with success, and again earn the gratitude of the whole astronomical world. The idea is so startling that comments may well be deferred; but there is one point on which a question, if raised at all, is best raised now. The epoch selected for the proposed Catalogue is 1875·0, the same as that for the Astr. Gesellschaft Catalogue. For comparison with the measures on the astrographic plates 1900·0 would be much more convenient. No doubt that point has been well considered, and there are excellent reasons for the choice. But the question I would raise is this: could not the R.A. and Dec. be given for 1900·0 *as well as* for 1875·0? It would be an immense boon to many; and, though the scheme is already a large one, I venture to think that this addition to it would not be regretted.

THE January number of the *Astrophysical Journal* contains an interesting paper by Prof. Newcomb, in which he discusses the question whether there is a perfectly uniform period underlying the approximately periodic phenomena of sun-spots; and he decides the question in the affirmative, giving the period in years as 11·13 ±·02, with maxima in 1871·52, 1882·65, 1893·78, 1904·91, &c.; and minima in 1878·03, 1889·16, 1900·29, 1911·42, &c. In the year 1889 Mr. Hirayama published, in this magazine (May 1889, p. 217), a list of "Supposed Sun-spots observed with the Naked Eye," from the Chinese Annals. I discussed them provisionally at the time to see whether these records, which go back in a fragmentary manner to A.D. 188, would perhaps throw any light on sun-spot periodicity, and came to the conclusion that a "mean period of 11·06 years suits the records, giving maxima in 189, 300 . . . . . . 1638, 1881" (*loc. cit.* p. 219); but the information yielded was far from decisive; and as it was by no means certain, or even probable, that the period was exact and uniform, the investigation was not pushed very far.

On reading Prof. Newcomb's paper I turned again to these records to see whether this new information would help; but I must confess that nothing very satisfactory seems to come from them. Those from 1077 to 1276 are curiously inconclusive: almost any epoch of maximum would suit them fairly well. Of course naked-eye spots are by no means confined to maximum, and this imperfection in the records may make it impossible to make use of them for the object in view. Carrying back Newcomb's epoch and period we should get maxima in 1381·80, 1370·67, 1203·72, 1181·46, 1136·94, 1081·29, 368·97, 302·29. A glance at Mr. Hirayama's table will show that these suit the Chinese records very well; and one is inclined to think Newcomb's period confirmed with great exactness. It is only on trying a slightly different period that we find how disappointingly little the exact period seems to affect the general agreement.

WE have had considerable excitement in Oxford during the last month concerning a request made by the Professor of

Astronomy for a house to be built adjoining the University Observatory, which is placed, as at Greenwich, in a Park. A large section of the University sees evil in any request made by any scientific man, and was up in arms at once. They were with difficulty persuaded that no expense to the University was proposed, although this was clearly stated from the first; and they were then sure that this was only the beginning of a movement for all scientific professors settling down in "Villas in the Park." This is the view adopted in the following poem from the *Oxford Magazine* of March 6, which may be taken as representative of a large amount of literary effort on the topic:—

To Congregation.

A weary place is Oxford town for wights that learnèd are,
And this among its thousand ills the hardest is by far:
They probe the heart of Nature's works with epoch-making ken,
Yet have to live in roads and streets, like ordinary men!

I soared into Futurity on Fancy's roseate wings:
I saw before my mental eye an altered state of things:
Professors of the Sciences were happier far than now,
For each within the Parks possessed three acres and a cow.

No wickets marred the peaceful spot where batsmen used to bat:
No persons played with hockey-sticks—there was not room for that:
No athletes skipped with manly grace about the herbage green—
Professors and their villas red monopolized the scene:

The Chemist, who employs his days inventing of a Stink,
The Animal Morphologist, who seeks the missing link,
Likewise the fierce Anatomist, who rends you limb from limb,
Each in the Parks a mansion had, designed and built for him.

No longer doomed his feet to drag for many weary miles
From Bradmore, Crick, or Norham Road, or e'en from far St. Giles,
No longer forced to cross the street, 'mid risks that nightly lurk,
By the Museum each one lived, adjacent to his Work:

Amid its wives and families in patriarchal state
Accomplished thus its daily tasks the Professoriate:
It scanned the stars through telescopes, with nightcaps on its head,
Dissected in its dressing-gown, and lectures gave in bed.

Thus lived and worked the Scientist in calm domestic bliss;
So when you vote (on TUESDAY NEXT) just fix your mind on this—
These bright and glorious prophecies the Future may fulfil,
And if you vote the proper way, assuredly it will!

In reply to this objection it was explained that no new ground was to be taken from the sacred Park, as the house was to be within the present Observatory enclosure; and the other scientific professors very kindly signed a declaration to the effect that the case of an astronomer was quite exceptional, and that none of them had any necessity for night-work which made a residence in the Park desirable. But it was found impossible to persuade 126 people that some nefarious scheme was not concealed by this apparently straightforward proposal: they were sure that if obvious objections were removed there must be some much more subtle one behind, and so they voted against the scheme; and as only 125 voted for it, the motion was lost by 1. Those who know the ways of the University say that the proposal may be again presented in a year or two with fair chance of success.

THE note of M. Ch. André in *Astr. Nachr.* No. 3698, on the light-variations of Eros, is sufficiently startling. He finds the total period of $5^h\ 16^m$ divisible into two periods of $2^h\ 51^m$ and $2^h\ 25^m$; and concludes from the light-curve that Eros is a *double planet*, resembling the double stars β Lyræ and U Pegasi, the components revolving round one another in close proximity in elliptic orbits. He has given the test for verifying his hypothesis (that the complex light-curve should gradually disappear), and we shall therefore await this verification with great interest. If it fails, some other hypothesis may be found to satisfy the facts. I suppose if the Earth were seen as a point of light the grouping of the two large patches of land would give it a light-curve similar to that described by M. André for Eros.

A WONDERFUL series of observations was made at the eclipse of May 28, 1900, by Charles P. Howard, at Winton, North Carolina *, in less than half a minute. He devoted 73 seconds to photography —"unfortunately," for "it was a great mistake to give so much time to photography and so little to the view through the wide field telescope." If the eye-observations lost through this mistake would have been like those actually made, the loss is certainly a serious one, for in the last half-minute Mr. Howard made some absolutely unique discoveries. Some of them "cannot be represented on paper nor described. For some reason it seemed as if the connection between mind and matter was in plain sight"; and Mr. Howard remembers saying to himself, "This is what we'll see after we are dead." This seems impressive enough, but it is not given the premier position in the observations. "Most conspicuous and wonderful of all" (is this because we shall *never* see them, even *after* we are dead?) "were eight slender white stalks having fan-shaped tops, that rose from each polar region to a height of about 180,000 miles. All sixteen were exactly alike, except that the curvature of the stalks was a little greater in those furthest from the poles. Their uniform breadth was about 3500 miles and each fan-shaped top was nearly an equilateral triangle 20,000 miles on a side; the outer side, however, was about 1000 miles shorter than the other two." Drawings are given of these wonderful things, but not photographs. Apparently they were not shown on the photographs, and Mr. Howard may well lament the time wasted in photography! Those who are able to obtain a sight of this pamphlet will read with almost equal wonder of what was accomplished in the last three seconds of totality, or rather in only two of them. A small reddish glare was at once recognized as a prominence out of focus. It was brought into focus, and its chief features noticed and measured. Five or six others were then seen near the Sun's equator: it was seen that the corona was now out of focus, though the white stalks were still visible, plainly out of focus; and "while the eyepiece was being moved in and out to focus the thread-like prominence, I particularly noticed that the white stalks went in and out of focus exactly with the corona proper." I do really trust Mr. Howard has gone out to see the eclipse of next May. We ought to know whether those stalks are still there, and he seems to be the only man who can tell us.

---

* Printed as a pamphlet by R. S. Peck & Co., Hartford, Conn.

Nova Persei seems to be fading fast. It has attracted much attention in the daily press, and many cuttings have come to hand. The following verses appeared (with four others not quite so good), over the signature S. Tomlinson, in the *Leeds Mercury* Weekly Supplement:—

The New Star, Nova Persei.

Oh, shining star, how bright you are,
With silvery surface glittering overhead;
The daylight past, how soon you cast
A ray of light upon the path we tread.

Although to such as don't know much
(That I am such I do regret to say)
About the skies, to our blind eyes
It surely seems as if you'd lost your way.

But do not mind if long behind
A million other bright lights in the hall,
You're not like some, who say they'll come—
"Oh, yes, we'll come!"—and never come at all.

Fragments of a letter received from Port Said, dated March 19:—

> Here we are in very pleasant circumstances on our Dutch boat, all hale and hearty. . . . . In the skurry, for which that infernal new star is in the main responsible, I forgot . . . . . We are all writing letters hard for posting at Port Said to-morrow. We are due there at about 3 o'clock. Our first day out of Genoa was grey, cold, and choppy; and meals were rather serious. But next day it was beautiful, and we passed Stromboli, Messina, and Etna in brilliant sunshine, and have had it each day since. The Dutch Captain is a very good sort, and the whole reception on board has been most cheery and hearty. It is a first-rate Glasgow-built boat, and we fare very well. Besides the Dutch party on board there is one of geodetical folk from the Boston Institute of Technology, who come to swing pendulums and observe magnetometers, and don't much care whether they see the eclipse or not. We hear there are no less than seven American parties going to Solok, and they are all ahead of us, so we can hardly expect to find a blade of grass left. . . . .

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 April 12.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: E. T. WHITTAKER, M.A., and W. G. THACKERAY (*vice* F. W. DYSON).

THE Minutes of the previous Meeting were read and confirmed.

The President. The first business is for me to announce that His Majesty the King was graciously pleased to signify his willingness to receive in person the address of condolence and congratulation voted by this Society. Accordingly, on March 20, in company with the Secretary, I presented the address to the King at St. James's Palace.

Mr. Whittaker. 88 presents have been received since the date of the last Meeting. Among them the following call for special notice:—enlargements of the Astrographic Chart of the Heavens, which have been presented by the French Government; Prof. Hale, Director of the Yerkes Observatory, has sent a photograph of the cluster in Hercules, and also a photograph of a portion of the Moon taken with the 40-inch telescope at the observatory; Prof. Barnard has sent a photograph of the solar eclipse of 1900 May, which is now on view in the office; a siderostat has been presented by Mr. Alexander Foote, and is on view in the Meeting Room.

Prof. Turner then read a paper by *Mr. H. C. Plummer* on a method of mechanically correcting the rotation of the field of a siderostat. At the January Meeting he (Prof. Turner) had the honour of reading a short paper on this subject, which had previously been studied by M. Cornu. Since seeing this paper Mr. Plummer had made a simpler suggestion for effecting the same object. (Prof. Turner then described the method proposed by Mr. Plummer on the blackboard.)

A vote of thanks was passed to Mr. Plummer and Prof. Turner.

Prof. Turner then gave an account of his paper relating to the reductions of the Astrographic Catalogue. It is probably well known that one method of dealing with astrographic plates is to convert the rectangular co-ordinates of the stars into "standard co-ordinates," which give the position which the star would occupy on the plate if there were no aberration, no refraction, and no correction to scale-value, by the simple addition of three numerical terms to each co-ordinate. Now the plates of the Astrographic Catalogue are so taken that corners of two overlap, and the same stars are seen on both plates. The measured co-ordinates of these stars may be compared (*a*) directly, or (*b*) by reducing each set to standard co-ordinates which are connected geometrically. The present paper gives formulæ and tables for facilitating method (*b*); for it is important to use both methods for detection of systematic errors. This is a kind of work rather unfamiliar, but I feel sure we are bound to have a good deal of it later on, so that I have spent some time on it.

The President. I should like to ask Prof. Turner whether I am to understand it is an original method of his own, or whether it is a well-known method, and the essence of the paper merely consists in adapting the formulæ?

Prof. Turner. There is nothing essentially original. It is rather the transformation of two sets of linear equations. It gives a method of dealing with large figures by a mechanical process.

Sir Robert Ball. Mathematicians who deal with recondite subjects such as homographic systems are sometimes told that their work is of no practical use, but here is a very beautiful practical illustration of two homographic systems. I should like to ask Prof. Turner if he has made any use of the three points common to the two systems. There are three points on each of the plates which have the same co-ordinates in the standard system as they have in the actual system.

Mr. W. W. Bryant explained his paper on the "Two Method Personal Equation." The paper discussed the differences of the personal equation when observing chronographically and when observing by the eye-and-ear method applicable to certain Greenwich observers. Certain progressive changes in the case of some observers were pointed out, as well as periodic changes, and changes due to the declination of the object observed, and hence to the position of the observer with respect to the observing clock.

Prof. Turner. This is a most interesting paper, and the comment I should like to make is with regard to an incidental remark made by Mr. Bryant. He referred to a paper by Mr. A. W. Roberts, of South Africa, on variable stars. I looked at that paper lately, and am surprised that it did not attract more notice. The method used by Mr. Roberts is so simple, and the piece of work done so valuable, that I should like to draw the attention of those who are looking for work to do to the matter. He was engaged in investigating the personal equation in estimating the magnitude of variable stars according to their position in the field relative to the comparison star. His method was always to make observations twice over—once with a direct eyepiece, and once with an inverting eyepiece. In this way he made a very complete investigation of his own personality in estimating the magnitude according to the position of the field, and he found the remarkable result that the critical direction did not take a horizontal position, but had an angle of 30° to that, and it is very important to know if it is the same with other people or not. It would be a most valuable thing in variable star work if that were known. It would be a piece of work that would be valuable if one were to observe the same stars with two different eyepieces for about a year. I am convinced that one has that personality, because, in examining observations of variable stars made by certain other observers, I found it out, but it is not easy to study the effect when the direct experiment made by Mr. Roberts is not made. All these questions of personal equations are very interesting, and although the one I have alluded to is rather off the line of Mr. Bryant's paper, I hope he will excuse the digression.

The Astronomer Royal. The difficulty that I feel about this is as to what is the method of observation with which we are dealing. Mr. Bryant has mentioned the muscular method, in which the observer waits until the star is on the wire, and then taps, but that is not the method which is practised by all observers. In chronographic observing the observer should regulate his action so as to make the sound of the tap correspond with the passage of the star across the wire. That is what I thought everyone aimed at. It is a question of training the muscles so as to make the sound of the tap heard coincide with the bisection of the star by the wire as seen by the eye. As to the eye-and-ear method, I am also not clear, from what has been said, as to what is the method of observation by different observers. The old plan was for observers to note the positions of the star when the beat of the clock is heard before and after the transit over the wire, and then to estimate the relative distances between these and the wire. I think that Mr. Bryant assumed it to be entirely a question of ear. There is a further interesting point which appears to have been lost sight of, to which M. Wolf has called attention. In his view personal equation is due to the circumstance that a star in its passage forms a line of light owing to the persistence of vision, and he found from the experience of practised observers at the Paris Observatory that there was a tendency to refer the star to one end or the other of this line, and that personal equation was caused by this idiosyncrasy.

The President (to Mr. Bryant). Are you sure the observers observed in the way you mentioned?

Mr. Bryant. It seems to me to make very little difference whether the division of the second is done by eye or by ear.

Mr. Hollis. In answer to that question, and as my personal equation is now being talked about, may I be allowed to explain exactly how I observe? In observing chronographically I wait until I see the star actually bisected by the wire, and then I tap. I am one of a very few, I believe, who do that, and so I naturally record the time of a transit later than most persons. I learnt transit-observing from Mr. Dunkin, and I think he always observed that way. He used to say "You must observe what you see, and not what you think you are going to see." In observing by eye-and-ear, I do what the Astronomer Royal has just described, I note the position of the star at the clock-beat just before transit and at the clock-beat just after, and I have somehow a distinct mental picture of the two positions of the star with the wire between them, and I seem to myself to estimate the proportions of the space with the same accuracy as that with which I read a micrometer-head. But I believe that I hear the clock *late*, and so I record the time in this case *early*, which will account for the difference between my personal equation when observing by the two methods that is shown on the board.

Mr. Bryant. There are two of the computers at Greenwich who observe by chronograph just as Mr. Hollis does, and their personal equation is larger than his.

The President. I am sure the Society will thank Mr. Bryant for his interesting communication, and will hope that he may have further opportunities of continuing his researches.

Mr. McClean made some remarks on his recent spectra of Nova Persei *. Photographs to the number of 30 had been taken of the spectrum of the new star up to April 10. Enlargements of those taken on February 27, March 3 and 12, and April 5 were mounted together and shown to the Meeting. The hydrogen absorption lines disappeared from the photographs between March 18 and 22. After that little detail was visible except the dark and bright bands between $H\beta$ and $H\gamma$.

Mr. Whittaker said that short papers had been received from *Mr. Stanley Williams* and *Mr. Sharp* on the magnitude of Nova Persei. Mr. Sharp sent a diagram giving his estimate of the magnitude of Nova Persei during the whole course of its visibility. Comparison between the results obtained by Mr. Williams and Mr. Sharp had been made, which showed some points of interest, and it would be interesting to compare both of them with the observations made at the Radcliffe Observatory. Mr. Williams and Mr. Sharp did not succeed in getting observations on the same nights, so that their work was to a certain extent complementary. On the nights when both got observations they were perfectly in agreement, but when Mr. Sharp did not get observations he made a conjectural curve to fill up his diagram, which was found to be far from the truth when compared with Mr. Williams's paper, because it appears that the actual changes of magnitude of the star had been so irregular, or rather they appear to some extent to be periodic *.

---

* Owing to a failure in the hydrogen supply, the lantern was not available for showing the slides which accompanied these.

Dr. Rambaut. I see from a comparison of our observations with those of Mr. Williams that we agree very closely. The first observer seems to have been unfortunate in the date of his observations. He seems to have missed the minima. The observations I now contribute have been made in continuation of those contained in the last number of the *Monthly Notices*, but some of the later observations had to be made with the telescope, as the star lost brightness. A number of apertures were used, varying from 1¾ to 10 inches. The observations of colour have some points of interest. The observations show that the two phenomena of brightness and colour are related. On March 21 the star was as bright as 3·91, and the colour is described as having a stronger yellow tinge than when last seen. On March 23 the Nova was noted as being very red. On March 26 the colour is described as red with yellowish tinge. In general the colour has varied in the red direction as the light of the star diminished, and the same thing seems to hold in the short periodic variation.

Mr. McClean. The magnitudes were approximetely estimated from neighbouring stars.

The President. Does Father Sidgreaves in his paper say anything about the minima brightness?

Mr. McClean. I do not know.

Sir Robert Ball. I am sorry to see that Dr. Rambaut was not able to get an observation on March 19, and I should like to ask him whether he does not think it was possible that there was another drop in the curve on that day. I happened to be in the north of Ireland, at Enniskillen, and I took some people out to show them the new star, but it could not be seen. It was evidently at minimum, and I ought to add that Perseus was low at the time. The next night, the 20th, I was in another neighbourhood, and I brought some people out to demonstrate the *disappearance* of the new star, and there I found it back again.

Dr. Rambaut. What Sir Robert Ball says has been corroborated at other places. The minimum was noted on the 19th by Dr. Glasenapp at St. Petersburg.

Mr. Whittaker then described a paper by *Mr. A. Graham*, of Cambridge Observatory, "A Note on Meridian Observations of Nova Persei."

Mr. Whittaker said that a paper had been sent by *Mr. Innes* from the Cape Observatory, South Africa, entitled "Anomalous Occultations of Stars by the Moon." Mr. Innes began his paper with the statement that it was possible to suspect that a star was double by observing its occultation by the Moon. If the star was double its two constituents were occulted, not at the same instant, but at instants which differed slightly from each other. The result was that the disappearance seemed to take place in two stages, or gradually. Mr. Innes said that apparently for physiological reasons the disappearance did not take place in two stages, but took place gradually. Mr. Innes gave a remarkable instance of a double star which was observed to be occulted in this peculiar way long before it was found to be double. This observation, which was made by Sir Thomas Maclear in 1836, was put on record, but it was not until 15 years later that Burnham found it to be a double star. Another remarkable example quoted was the star *o* Tauri. He concluded his paper by saying that probably a considerable number of the stars suspected of being double from their appearance at occultation would be too close to be visible to the telescope. The great point in favour of these occultations was that they could be observed with telescopes of very moderate size. Mr. Innes proposed to make a collection of records of such anomalous occultations.

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* See page 206.

Mr. Bryant. I think you will find it is large O Tauri, and not omicron Tauri. I have looked it up.

Mr. Whittaker. He spells it out here—omicron.

Mr. Knobel. The suggestion made is a very old one. I well remember, in the early sixties, a discussion taking place with regard to *κ* Cancri—as to whether it was double; but the allegation was refuted by Dawes, who observed its instantaneous disappearance at an occultation. I think this discussion took place in 1865 or 1866.

Mr. Whittaker described another paper that had been received from South Africa, namely, from *Mr. A. W. Roberts*, on "Variations of R Horologii in 1900." Mr. Roberts called attention to the fact that the light changes considerably, and that at its maximum the star was at least 650 times brighter than at its minimum.

Mr. Whittaker said that *Mr. Denning* had sent a paper on a meteor from the region of *α* and *β* Persei and *η* Aurigæ. Mr. Denning compared the observations from different places, and found as a result that the object fell from a height of about 84 to 27 miles above the English Channel, at a point 15 miles from St. Catherine's Point. He compared its radiant with the radiants in his catalogue, which was published some years ago in the Memoirs of the Society. He found that it did radiate from one of the well-known radiant-points of showers. Mr. Denning said that it was interesting to note that the position of this radiant was exceedingly close to the Nova in Persei.

The following papers were announced and partly read :—

Rev. J. G. Hagen. "Note on some Engraved Charts of Pogson's proposed Atlas of Variable Stars."

W. F. Denning. "Meteoric Showers from the Region α–β Persei and η Aurigæ."

R. T. A. Innes. "Anomalous Occultations of Stars by the Moon."

H. C. Plummer. "A Method of Mechanically Compensating the Rotation of the Field of a Siderostat."

A. W. Roberts. "Variations of R Horologii in 1900."

A. Graham. "Note on Meridian Observations of Nova Persei."

A. Stanley Williams. "Further Observations of the New Star in Perseus."

Rev. W. Sidgreaves. "The Spectrum of Nova Persei, Note 2."

Rev. W. Sidgreaves. "The Spectrum of Nova Persei, Note 3; Nova Persei as a Variable Star with a Variable Spectrum."

M. C. Sharp. "Observations of Nova Persei."

G. M. Seabroke, H. P. Highton, and E. C. Atkinson. "Further Measures of Double Stars made at the Temple Observatory, Rugby, during the Years 1895–1900."

Radcliffe Observatory, Oxford. "Observations of the New Star in Perseus."

H. H. Turner. "Formulæ and Tables for correcting Co-ordinates of Stars on Different Photographs, especially different Plates of the Astrographic Chart."

W. W. Bryant. "Further Investigation of the 'Two-Method' Personal Equation."

The following gentleman was elected a Fellow of the Society :—

The Rev. Richard Coad Pryor, M.A., The Rectory, Grafton Regis, Stony Stratford.

The following Candidates were proposed for election as Fellows of the Society :—

Francis William Crook, B.A., Barrister-at-Law, Secretary to the Technical Education Committee of the Kent County Council, 4 Overcliff, Gravesend (proposed by Sir D. L. Salomons).

Frank Lowman, B.A., Lecturer in Science, &c., St. John's College, Battersea, S.W. (proposed by A. E. Moore).

Charles Nielsen, 15 Cliff Terrace, Hartlepool (proposed by Sir R. S. Ball).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fifth ordinary Meeting of the current Session of the British Astronomical Association was held on Wednesday, March 27, at Sion College, *Mr. G. M. Seabroke*, the President, in the Chair.

The names of five Candidates for membership were read and passed for suspension, and the election by the Council of nine new Members was confirmed.

Col. E. E. Markwick (Director of the Variable Star Section) contributed a note on the Nova Persei. He stated that he had reduced all the observations of brightness of the Nova, made principally by Members of the Section, according to the R.H.P. scale, and plotted them on squared paper. The results of an inspection of the light-curve might be briefly stated. From below the 11th magnitude on February 19 the light shot up to 2·7 mag. (Dr. Anderson's observation on discovery) on the 21st. It continued to increase, and the maximum was most probably attained on February 23, 0·3 mag. After this the light fell away rapidly, and the object was of mag. 1 on February 25, mag. 2 on March 1, mag. 3 on March 7, and mag. 4 on or about March 18. During the past few days the variations, according to his observations, had been rather anomalous. Between the 18th and 19th March the star fell quite a magnitude and a half, from 3·71 to 5·25 mag. On the 22nd it was still 5·25, but on the 24th it was rated 4·0, so that it had risen a quarter of a magnitude between those dates.

Mr. Henry Ellis stated that he had obtained a number of photographs of the Nova. In examining the plates it was very noticeable that the image of the star was large, woolly, and ill-defined as compared with an ordinary star. He attributed this to the large gaseous envelope which the spectroscope appeared to indicate.

Mr. M. C. Sharp contributed some notes on the colour of Nova Persei.

Mr. W. T. Lynn estimated the star at below the 5th magnitude on March 25 as seen at Blackheath.

Mr. Brook said he had only been able to see the star on two consecutive nights. On the 25th, at 11 P.M., he made it out to be mag. 5·2 on the Oxford scale. On the 26th it was certainly brighter; he estimated it at 4·5. At 11.30 it appeared to be still brighter, to the extent, perhaps, of two-tenths of a magnitude.

Mr. Crommelin said he could fully confirm what had been said as to the variation in the brightness of the star between March 25 and 26. He was fortunate enough to get a clear glimpse on both occasions, and his own estimates, before receiving Col. Markwick's telegram, were: 5·2 on Monday, and 4·2 on Tuesday, at 11 P.M., on the Harvard scale. He had received a letter from the Rev. T. E. R. Phillips, in which that gentleman put it at 5·5 on Monday night, and said it was quite a magnitude brighter on the following night at 9.30. It could therefore be taken as established that there had been this sudden rise. As to colour, Mr. Lewis was observing the star the previous night with the 28-inch telescope, and he said he had never seen any star so red. It appeared, however, absolutely stellar—he could not see any fuzziness about it.

Mr. Goodacre thought there could be no doubt about the fluctuations which had taken place in the brightness of the star. On Monday night (March 25) he could barely get a glimpse at it with the naked eye, whereas on the following night it was easily seen.

Mr. Maw thought there must have been some changes last Sunday night within a very short period of time. He examined the star at 8.30 on Sunday night, and could only see it after he had set the telescope on the star and looked along the top of the finder. At 9.30 he was able to see it quite clearly with the naked eye. He was rather struck with the change in the spectrum between the 11th and 24th. It had, of course, become very much fainter, but it was not regularly fainter. The bright red C line was proportionately quite as bright as on the 11th, and so was the G line; but all the lines between those two were very much fainter. He thought members would be interested if they tried to compare the out-of-focus image of the Nova with the out-of-focus image of another star of about the same magnitude. There was a brilliant red colour in the out-of-focus image of the Nova, and the whole appearance was totally different from that of the corresponding image of an ordinary star.

Mr. Henry Ellis said he could entirely confirm the last point of Mr. Maw's remarks.

Several other members joined in the discussion, after which *Mr. Chambers* read a letter from a *Mr. A. Elvins*, of Toronto, on the connection of storm-periods and rainfall in reference to the sun-spot period. The writer stated that at Toronto they had a maximnm of cyclonic storms at sun-spot maxima, but in the case of rainfall they had two waves of great rain in each sun-spot cycle. Sir Norman Lockyer had observed the same in the rainfall of India and Mauritius.

The Rev. A. L. Cortie (Director), presented an interim report on the Solar Section, dealing with the solar surface during the year 1900. The general characteristics of the spots observed during the past year were their small size and rapid evanescence. There was no example of a group remaining long enough on the solar surface to reach the phase of a single dark round spot. On August 29 and 30 Mr. Hadden observed a small group recorded as being in a high south latitude. The appearance of this group, joined with the rapid decline in solar activity, would seem to indicate that we are now actually at the minimum.

Mr. Crommelin said it was well known that the beginning of a cycle was usually signalized by an outbreak of spots in high latitudes. He remembered this was the case in 1889. One of Father Perry's last observations, published in the *R. A. S. Monthly*

Notices for November 1889, before he went on the eclipse expedition that ended fatally for him, was the outburst of a group in high latitudes, which was the beginning of the cycle that culminated four years later (in 1893). So that they might perhaps look upon this spot in high latitudes as being the beginning of a new cycle; and if it represented an eleven-year period, they might look for the maximum about 1904 or 1905. It was rather curious that the two Spanish eclipses gave opportunities of studying the Sun probably at extreme minimum and extreme maximum. Last May they had the Sun practically at extreme minimum. The next Spanish eclipse, in 1905, would give, as far as could be foreseen, a corona of maximum type, and it would be very interesting to compare the two. There had been a paper recently published in the *Monthly Notices of the R. A. S.*, suggesting that the solar cycle was due to Jupiter and Saturn, and the theory certainly explained a good many of the waves in the solar cycle. According to this theory the next maximum would be delayed until about 1908. They had an opportunity of testing the theory by seeing, when the next maximum came, whether it repeated the eleven-year cycle, or whether it would be delayed two or three years longer.

Mr. R. Hodge exhibited some photographs (with the aid of the lantern) illustrative of his observatory at Stroud Green, more particularly in order to show how he manipulated the roof and sides. He said he had found duraline an excellent covering for the roof.

The President thought that ordinary canvas, repainted every two or three years, made the most satisfactory roof.

Mr. Goodacre said that a canvas roof, repainted every two or three years, was practically everlasting.

Mr. Holmes read a translation of a letter he had received from M. Peridier, of Paris, advocating the graphical method of explaining celestial motions "to beginners not satisfied with the pedantic explanations of the majority of our manuals."

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, April 17, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. W. H. Dines*, President, being in the Chair.

Mr. W. Marriott read a paper on "The Special Characteristics of the Weather of March 1901." Nearly every one will remember how bitterly cold and uncomfortable was the weather of last month, and in this paper the author brought out in a very graphic manner its chief features. From March 1st to the 12th or 13th the temperature was slightly above the average, the prevailing winds being from the south-west and often strong in force. About the 13th a change set in, when north-easterly winds became predominant and low temperatures prevailed. This continued with increasing intensity until the 29th, the last two days of the month being nearly of average temperature. The most remarkable period of the month was the five days from the 25th to the 29th, when the temperature was more than 10° below the average all over the country. The north-easterly winds were strong in force and particularly keen and dry. At the Greenwich Observatory the relative humidity was only 52 per cent. on the 26th, and 54 per cent. on the 27th. The only other instance during the past 54 years of as low a relative humidity in the month of March was on March 1, 1886.

In consequence of this keen and cold weather vegetation was at a standstill. Snow showers were frequent, but not very heavy, except on the 20th, in the south-west of England, when on Dartmoor nearly as much snow fell as in the great blizzard of March 1891, and on the 29th, when a very heavy fall of snow and rain occurred in the north-west of England and Wales.

Although the death-rate was below the average, there was a considerable increase in the deaths due to diseases of the respiratory organs.

A paper by *Mr. R. Strachan*, on "Vapour Tension in Relation to Wind," was also read.

The New Star of the New Century.

THE early history of the new star in Perseus is too well known to astronomers, both amateur and professional, to need repetition. But an outline of the successive stages of its early growth and beginning of decline is necessary for the connection between them and the subsequent changes observed in the failing and dying star.

From the Harvard College frequent charts of the sky we know that the Nova was practically not in being on February 19. In the small hours of the morning of February 22 it was first observed by Dr. Anderson, and was then between the third and second magnitude. At 7 o'clock in the evening of the same day it was seen by Dr. Copeland as a bright white star of nearly the first magnitude, with a strong continuous spectrum streaked with very fine absorption-lines. A few hours later it was observed at Harvard College as a white star of 0·9 magnitude, with 30 dark lines crossing its strong continuous spectrum. Later in the same night its magnitude rose to 0·35, and on the following evening it seems to have reached its greatest brilliancy at a probable magnitude of 0·0, but its spectrum could not be observed. On the 24th the star was found, both at the Edinburgh Observatory and at Harvard College, to have lost in brightness and to have changed its spectrum. On this night the spectrum appeared with the array of bright and dark bands familiar to observers of Nova Aurigæ in February 1892. The decline had begun, and the spectrum had settled to a comparatively permanent type, as shown by many spectrographs and by notices from private observers.

On February 28 the star was seen for the first time at Stonyhurst, and its spectrum was observed and photographed. Since then spectrographs have been obtained on March 3, 7, 12, 16, 17, 20, 21, 22, 25, 27, 28, and on April 1, 4, 5, 9, 11, 12, 13, 16, 20, and the rest of the star's history is drawn mainly from these photographic records.

On February 28 and March 3 the bright hydrogen lines were very strong and very broad, their breadth being quite 30 tenth-metres. These retained their general character of great breadth and relative strength up to the last. But the breadth of the dark hydrogen lines remained uncertain until March 7, when a shorter exposure (quicker trail) revealed the red side margins of the dark lines as quite distinct from the blue side edges of the bright bands, and showed the lines to be then about one-fourth the width of the bright bands. They have since then slowly thinned away, first losing their centres by apparent well-defined reversals; then the violet side component of the double line dwindled and disappeared, and the remaining line followed in decline, being seen for the last time on March 21. The calcium line K, which was broad and bright on February 28 and March 3, fell off nearly synchronously with the dark hydrogen lines, preceding by a few days: it was quite extinct on March 16.

This is the history of the star up to March 21, omitting details of uncertain interpretation. On March 22, on first sighting the constellation through an opening in the clouds, the impression was that the Nova was gone. But it was soon found in the field of the prismatic camera, and it there showed an unwonted brilliant yellow band, competing with $H\alpha$ and $H\beta$ for precedence in lustre; and on the photograph appeared an equally-prominent blue band with paler companions between $H\beta$ and $H\gamma$, on a much enfeebled continuous spectrum; and lastly a remarkably strong extension of the bright $H\zeta$ band, quite doubling its ordinary width. This eventually appeared to be a periodic spectrum. It was repeated on two very good photographs of the 25th and 28th, while on an intermediate plate of the 27th the spectrum was very closely the same as on the 21st, with the blue bands nearly masked by the continuous spectrum, and the $H\zeta$ band at its usual width. It was also noted that these dates, March 22, 25, 28, were dates of minimum brightness of the star, showing the same three-day period, and systematic measures of magnitude at several observatories have given the dates of minimum light March 19, 22, 25, 28, 31, and April 6. But the latter minimum was prolonged to the 7th, according to the measures of the Oxford Radcliffe Observatory, and after this date the periodic variation has become uncertain. From unaided eye-comparisons the star was less than *l* Persei on April 11, 12, 16, 20, 21, and on these dates the spectrum was of the minimum type; it was brighter than *l* Persei on April 1, 4, 5, 9, 13, and the spectrum was of the non-minimum type. The spectrum therefore follows the light-curve, whether this is connected or not with any periodic occurrence, and two inferences can be drawn from the changes: 1st, that there was for a time a periodic recurrence of some cause dimming the white light of the star without stopping its gaseous radiations; and 2nd, that the early interpretation of the region between Hβ and Hγ * in favour of bright bands on a comparatively weak continuous spectrum was the true one.

So far the history of the star seems to be that an extensive mass of cold matter was rapidly raised to a high temperature, passing successively through the stages of the white-hot solid and liquid states, the incandescent vapour state, and possibly the dissociated gaseous state. The changes were rapid, and our weather in both hemispheres unfavourable. We have seen the spectrum only at intervals too far apart to follow all the successive changes. In its brightest time it was seen, as already noted, as a strong continuous spectrum with only very fine absorption-lines; and a few hours later the absorption-lines were stronger and more numerous, showing the development of the vapour state, and then the subsequent changes were hidden from us until much of the white-hot mass had passed into the gaseous state, showing bright bands together with dark lines.

A resulting extensive incandescent gaseous atmosphere about a comparatively small photosphere, as suggested by Prof. Scheiner for γ Cassiopeiæ, would suit the later days of the Nova's life; and a dark revolving companion, small compared with the glowing atmosphere and comparable to the photosphere, would meet some of the requirements of the periodic changes in the spectrum; the dark star could eclipse the white photosphere without stopping much of the more extensive gaseous radiation.

This would account, in a simple way, for the apparent strengthening of the blue bands, by greater contrast on a darker ground. But the new appearance of the bright band adjoining Hζ at these times would need an improbable complex arrangement of moving bodies to unveil this illumination at the minimum of general light. There is no room here for contrast illusion; the bright band when present has always been much brighter than its close neighbour Hζ, stronger even than Hϵ; it has more than once been equal to Hδ, and on April 20 it was the strongest band on the photographic plate, excepting only a line of maximum strength in the Hγ band; and at the brighter phases of the star it has been completely extinct even when Hη was present. It either comes into being at the minima, or is completely absorbed at the maxima of the light-curve.

In the earliest days of the star, whence came the immense and rapid evolution of heat, which changed its condition from darkness to the Orion-type brilliancy in less than 48 hours? Not from internal convulsions. We may be unable to set limits to the possible storms in hot stars, but we cannot admit such latent capabilities in a cold mass. Arrested motion is the most natural supposition for the mechanism of the universe; and collision is not only possible, but commends itself as probable by the apparitions of new stars always in the Milky Way. Here there are luminous stars enough for many hits, and there are probably as many more dark ones. The results of impacts between stellar masses can hardly be imagined; and it would be rash to deny the possibility of the very high velocities imparted to the scattered fragments, which are needed for the application of the Doppler principle to the spectra of new stars. A near approach would be a more frequent occurrence; and here, again, we cannot assign limits to the atmospheric convulsions at the rush-past. But in neither of these possible occurrences can be found a satisfactory cause of the shift of the dark lines to the violet sides of the bright companions, without attributing the dark system to an approaching star—a postulate which the persistent repetition of the same-way displacements in at least three successive new stars has greatly discredited.

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* *M. N.*, March 1901, p. 336.

The history of the present New Star, and the specialities of its spectra, taken without reference to its predecessors, are for the most part in harmony with the more probable occurrence in space of a compact body moving at high velocity through an extensive region of cosmic matter, as proposed by Professor Seeliger in 1892 *.

The chief objection to this explanation of the spectrum of Nova Aurigæ was that if the observers at Potsdam had succeeded in assigning the true positions of the wave-length centres of the bright and dark bands, it was necessary to postulate very high velocities for both the star and the cosmic cloud, opposite and in the line of sight. Nova Persei needs only the velocity of the compact star through the cloud at comparative rest. In the spectra of this star the wave-length centres of the bright and dark bands are provided for us by central reversals; and when those of the bright bands are matched with the hydrogen lines of comparison stars, they are found to fit them very closely, while those of the dark series, when matched in the same way by superposition of dark $H\beta$, show increasing intervals from $H\beta$ of the lines of shorter wave-length, giving a longer dark line spectrum. The longer spectrum indicates approaching velocity, and the same length indicates *nil* velocity relatively to the comparison stars.

The compact star rushing towards us through the stationary cloud would show at first nothing but a continuous spectrum, until the surface became heated sufficiently to form an absorbing or reversing layer, giving the addition of dark lines displaced to the violet side; while the torn-off particles of heated vapour would quickly lose their initial velocities, coming to rest in the cosmic cloud, as the luminous train of its miniature the shooting-star. Sufficient breadth might be given to the absorption-lines, which were never excessively broad, by the initial density of the vapour-envelope. This would subside by expansion, and the dark lines would thin down and disappear; while the bright lines would remain for a longer time, as in Professor Seeliger's illustration the train of the shooting-star survives the more brilliant nucleus.

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* *A. & A. P.* xi. 904.

The great breadth of the bright hydrogen bands would have to be attributed to very high velocities in all directions of glowing hydrogen, and these could only be brought about by the gravitational effect upon the surrounding cosmic matter. Hydrogen would probably be abundant in the cloud, and there would be meeting streams upon the photosphere producing density, and whirlwinds of great strength.

Lastly, the variations of the star's magnitude do not appear to offer any serious difficulty; for, as Professor Seeliger has noted, changes of this nature, and changes in the spectrum, are to be expected while the star is in the cosmic cloud. And it may be added that even the periodic changes observed in Nova Persei only need the addition of a revolving companion: a circumstance too probable in itself to injure the simplicity of Seeliger's hypothesis.

Thus so long as we restrict the discussion to a qualitative analysis of the spectrographs, all the phenomena find their explanation in a very probable occurrence in the crowded region of the Milky Way, except the periodic appearance of the bright band adjoining Hζ. But when we come to consider the measured width of the bright bands of Nova Persei, the great separation between the centres of these and the centres of the dark companions, and further compare its spectrum with the spectra of preceding new stars, it must appear that we have only been building up to pull down. The enormous velocity required to serve as the cause of the elongation of the dark line spectrum is enough of itself to shake one's confidence in the Doppler effect as the real cause of either the width of the bands or the elongation of the spectrum. And now that we are forced, by the repetition of the same spectrum of at least three successive new stars, to look for a general law according to which the velocity of the dark line source is always towards us, we can do little more than wonder and wait. But it seems that it must at least be admitted that a doubt has been cast upon our most cherished conclusions of stellar velocities in the line of sight by the great displacements exhibited in the spectra of the New Star of the New Century.

Stonyhurst College Observatory, W. SIDGREAVES.
1901, April 24.

Meteoric Showers in the Southern Hemisphere.

THE meteoric streams directed from radiants in the southern sky are little known, with the exception of those within 30° of the equator, most of which have been seen by European observers. The very great majority of the showers having radiants of more than 30° S. Declination still await suitable observations. It is true that Neumayer at Melbourne recorded about 2000 meteors during the years 1858–63. and that Heis determined 39 radiants from them, but these results were very meagre and unsatisfactory. What is required is a searching watch of the southern sky, prolonged over several years, by an observer of experience in, and special aptitude for, this branch of work. The field being practically unexplored, there is a mass of valuable and interesting materials to be gleaned by any one who will essay to gather them. It is fair to assume that some really important showers exist in the firmament of the southern hemisphere. The Perseids of August, the Quadrantids of January, the Andromedes of November, the Lyrids of April, and the Geminids of December are all situated in more than 30° of N. declination, and it is highly probable that several equally rich systems are visible from radiants far south. They have hitherto escaped discovery because they have been unsought for, but they must quickly become the prizes of the first individual who prosecutes a thorough watch for them.

Of the streams which European observers have already distinguished within 30° S. of the equator, there are several which exhibit more than ordinary strength, as, for example, the Aquarids of May and July, and the Piscids Australids of July. But the radiants are comparatively low as viewed by northern observers, and the displays are therefore presented under a marked disadvantage. Lieut.-Col. Tupman pointed out some years ago that the apparent richness of a meteor-stream varies considerably with the altitude of the radiant. The horary number of meteors for a shower in the zenith may be computed by multiplying the horary number observed by the zenith-distance of the radiant. Thus if a radiant in altitude 18° furnishes 15 meteors in an hour, it will furnish 49 in the zenith; if a radiant is in altitude 27° it will yield 33, and so on. It is obvious from this consideration that observers in England and other latitudes far north cannot witness southern showers to the best effect, and that many of the most feeble of such systems must elude their notice altogether.

In the hope that an energetic and sustained effort will be made to improve our knowledge of these streams, I subjoin a reference-list of some of the most prominent that have been already distinguished between the equator and 35° of south declination. There must be hundreds of others distributed amongst the well-known constellations of Centaurus, Crux, Argo Navis, Pavo, Grus, Lupus, &c., &c.

Chief Meteoric Showers observed in the Southern Sky.

	Radiant.			Radiant.	
	α.	δ.		α.	δ.
	°	°		°	°
Aug.–Sept.	4	−16	April	217	− 9
July	14	−16	April	218	−31
January	17	−14	April–May	226	− 5
November	34	−14	May–June	250	−21
January	56	−12	June–Aug.	250	−34
October	86	−11	May–June	254	− 3
Nov.–Dec.	86	−32	June	268	−12
Oct.–Nov.	98	−10	June	268	−24
Jan.–Feb.	104	−27	June	274	− 3
Oct.–Nov.	121	− 1	July	283	−14
December	136	−20	July–Aug.	287	−24
November	150	−11	July–Aug.	304	−14
April	165	− 6	July–Aug.	318	−10
April	165	−15	Aug.–Sept.	322	−22
April	189	−31	July	329	−14
April	190	−20	April–May	334	− 2
March	192	− 6	July–Aug.	339	−11
March	203	−28	July–Aug.	339	−27
April	205	− 9			

The dates given do not apparently cover the whole periods of visibility, but are intended to refer to the most active times. Some of these radiants are rather doubtful, and the positions need corroboration. An observer in a favourable locality would soon be able to re-determine the centres of nearly all the showers in the list, especially if he watched during the whole or nearly the whole of clear moonless nights. The visible aspect of meteoric streams generally appears to be one of great tenuity, and many of them will only meet with satisfactory recognition when the heavens are watched during very long periods.

A great number of radiants do not yield more than one meteor in 7, 8, or 10 hours, within the sphere of vision commanded by one observer. And it is necessary that the observations should be very accurate as well as durable, for there are probably more than 100 different radiants in play on any and every night of the year, so that the positions being often in close contiguity and sometimes overlapping, only admit of individual determination when the path-directions of the meteors have been very exactly recorded.

Bishopston, Bristol,
1901, Mar. 18.

W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Confirmed or New Variable Stars.

Gentlemen,—

From my observations I can confirm or report the following orange stars as certainly variable, though they do not appear in Chandler's 'Third Catalogue of Variable Stars,' excepting that B.D. 51°·762 is (1285) in the list of unconfirmed stars at the end of that publication.

1. Es.-Bm.	2. Bm.	3.	4.	5. R.A. 1900.	6. Decl. 1900.	7. Mag.	8.	9. Spectrum type.
				h m s	° ′			
		B.D. +51°·762	Perseus	3 34 7	+51 12	8·9	ROr$^{4\cdot2}$ *	
174	137	6	Gemini	6 6 16	+22 56	6·7	ROr$^{4\cdot3}$ *	III.
		B.D. +23°·1243	„	6 5 25	+23 14	7·3	YOr$^{2\cdot4}$	
634	521	B.D. +43°·3425	Cygnus	19 53 58	+43 59	8·2	ROr$^{3\cdot9}$	IV.

* These tints are midway between ROr and OrR.

Col. 7 gives the B.D. magnitude; col. 8 the average colour from my observations, the notation being the same as that in my letter on p. 278, *The Observatory* for July 1897; col. 9 gives the type of spectrum as given by Krüger in his 'Catalog der farbigen Sterne.'

The variation of these stars, so far as observed by me, is slight.

The magnitude of 6 Geminorum is usually nearly uniform, but at times it becomes decidedly fainter, its range being at least 0·6 mag., making full allowance for errors of observation. My observations on its colour also appear to vary more than can be accounted for by errors of observation.

The range of B.D. +23°·1243 is ½ mag., namely, from 7·4 to 7·9 mag.

Subsequent observations confirm the variability of Bm. 18 Piscium, announced by me in July 1897 as apparently variable.

Sunderland, 1901, April 16. T. W. Backhouse.

P.S.—Since writing the above I find that Birmingham 211, Cancer, is now brighter than usual.—T. W. B.

The Velocity of α Persei in the Line of Sight.

Gentlemen,—

Mr. Newall's announcement of "the Variable Velocity of α Persei" in *The Observatory* for December 1900 expresses a hope that others will secure observations of this star. Four observations obtained with the Mills spectrograph in the years 1896 to 1898 were published in the *Astrophysical Journal* for October 1898. An additional observation was secured in 1899, and five others have been secured since the publication of Mr. Newall's announcement. The following table contains all the Mills observations, with the time-intervals between successive dates:—

Date.	Velocity. km.	Interval. days.
1896 November 11	−2·0	
" 12	−1·8	1
1897 January 19	−3·5	68
1898 July 12...............	−2·1	539
1899 September 25	−1·5	440
1900 December 16	−2·0	447
" 26	−2·0	10
" 30	−2·0	4
" 30	−3·0	0
1901 January 13	−2·0	14

These ten observations, made in six different years, exhibit a range of only two kilometres. Since this is about the usual range in our results for such stars (due largely to changes in the observers' personal habits in measuring the plates), we may say that these observations afford no evidence of variable velocity. Several of the later ones cannot be harmonized with Mr. Newall's periods of 16·8 or 4·2 days. It may not be out of place to call attention to the fact that the intervals between observations are not only those given in the last column, but also the sum of any number of successive intervals.

Mr. Newall's velocities range from −4 to +8 kilometres. Their mean value is +1·7 km., about 4 km. above my value. His velocities of Capella are on the average about 3 km. below my values. This is but one of many cases illustrating the necessity and the difficulty of eliminating systematic errors from line-of-sight work.

Lick Observatory,
1901, Mar. 15.

W. W. Campbell.

The Comet of A.D. 1737.

Gentlemen,—

Amongst the numerous publications of Whiston, who succeeded Newton in the Lucasian Chair in 1701, was one which appeared in 1737, bearing the long title 'The Astronomical Year, or an Account of the many remarkable Celestial Phenomena of the Great Year 1736, particularly of the late Comet, which was foretold by Sir Isaac Newton, and appeared at its conclusion.' It must be remembered, in reading this title, that the year is taken as commencing, by the old style, on March 25, and Whiston, in enumerating the large number of celestial phenomena which occurred in 1736, includes amongst them an occultation of Aldebaran which took place just at the end of it by that reckoning, or what we should call the night of March 24, 1737; somewhat inconsistently, however, he also includes an eclipse of the Sun on March 1, and a total eclipse of the Moon on March 15, 1736. The comet to which he alludes, and with respect to which he claims for Newton the first successful prediction of the return of a comet, was seen in the first three months of 1737, having been first noticed by Dr. Rose Fuller, F.R.S., at Spanish Town, Jamaica, on the 26th of January (February 6 N.S.); the discoverer says "about Jan. 26," but that it was then so bright that it must have been visible before. Dr. Kearsley observed it at Philadelphia on January 27. Apparently it was first seen in Europe (at Gibraltar and Lisbon) on January 29, not in England until February 4 (15 N.S.) in the west part of the country, and on the following day at West Horsley, in Surrey, soon after which it was noticed to have a tail 2½ or 3 degrees in length. Whiston did not succeed in seeing the comet himself (though on one occasion he repaired to Hampstead in hopes to do so through its purer air), and the Astronomer Royal (Dr. Halley) was, he tells us, equally unsuccessful. But Bradley, then Savilian Professor of Astronomy at Oxford, obtained a series of observations there extending from February 15 (26 N.S.) to March 22 (April 2 N.S.). Cassini observed it at Paris until the same day as Bradley's last, but Manfredi, at Bologna, succeeded in following it until April 6. Bradley calculated its orbit from his own observations, and says of the result that "we may reasonably conclude that it cannot differ much from the Truth, and must therefore be near enough to enable future Astronomers to distinguish this Comet upon another Return, and thereby to settle its Period, which I cannot at present pretend to do, not having met with an Account of any former Comet that seems likely to have been the same with this, whereof a Description has been given particular enough to determine this Point." It would appear that Bradley considered that the observations were not sufficiently extended to indicate ellipticity by themselves, and that such prediction could only be founded on the method applied by Halley, of comparing the orbital elements with all others which had been calculated. But Whiston's statement that the return of this comet had been predicted by Sir Isaac Newton seems to have been, on the part of the latter, a mere conjecture, and therefore doubtfully expressed. He says that he had been heard on more than one occasion to say in conversation that he thought a comet would return in 1736, but when pressed about it drew back as though sorry he had said so much, but could not deny that he had formed such an expectation. This, Whiston thinks, was founded on a supposed identity between the comets of 1532 and 1668 (the elements of both of which are very uncertain), and his idea that the period was probably not 136 years, but the half of it, or 68 years, which would bring another appearance in 1736.

Although Bradley's orbit was calculated entirely from his own observations, and a somewhat longer range might be obtained by including those of others, it does not appear, so far as I know, that anyone else has determined it since.

Yours faithfully,

Blackheath, 1901, April 6. W. T. Lynn.

Solar Eclipses in the Cycle of that of 1901.

Gentlemen,—

At a time when so many of our thoughts are directed to the great eclipse of next month, it will perhaps be of some interest to refer to those, past and future, which belong to the same cycle of 223 lunations, or 18·03 years.

The first of these, on 1847 April 15, was total over a short tract of the Indian Ocean and the northern portion of Australia; but I have not been able to find any record of its observation.

The second took place in 1865, on the 25th of April; the line of totality passed across South America, the Atlantic Ocean, and a portion of Western Africa. Mrs. Todd, in her 'Total Eclipses of the Sun' (2nd edition, p. 129), reproduces a sketch of the corona taken on this occasion in Chili by Cappelletti, which is given in the Meteorological Bulletin of the Collegio Romano (iv. 89).

Eighteen years afterwards, in 1883, we are within range of the time (which may be said to have commenced with the great Indian eclipse of 1868) when astronomers were determined to lose no opportunity of augmenting our solar knowledge on the occasion of a total eclipse, in whatever part of the world it should occur, provided a portion of this is on *terra firma*. The only opportunity of observing that of 1883 May 6 (a very tempting one because the duration of totality exceeded five minutes) was on Caroline Island, a mere coral atoll near the Marquesas Group, in the Pacific Ocean. Successful observations were obtained there both by American and European parties (M. Janssen being with the latter), but into the detail of these we need not enter here.

Next month we shall be at the end of the next cycle, and by the end of it we shall hope to hear that our friends and other astronomers have been favoured with fine weather on the 18th at all or most of the localities at which they have taken up their stations.

In 1919 a total eclipse of the Sun will occur on May 29, the track of the shadow passing from the north part of South America across the Atlantic to Central Africa; where longest, the duration of totality will amount to six minutes.

Of all the later ones belonging to this cycle the paths of which are given in the maps in Oppolzer's 'Canon der Finsternisse,' two only, those of 2081 September 3 and 2135 October 7, pass over any part of Europe; the former will be total in France, North Italy, and Turkey: the central line of the latter will pass over England and Germany. Those of 1955 June 20, 2009 July 22, and 2081 September 3 will be total in India; those of 2045 August 12, 2099 September 14, and 2153 October 17 in the United States of America. Yours faithfully,

Blackheath, 1901, April 9. W. T. LYNN.

OBSERVATORIES.

STONYHURST COLLEGE.—Father Sidgreaves's neat little annual for the year 1900, containing chiefly the results of the meteorological and magnetic observations made at Stonyhurst, and the meteorological observations made at St. Ignatius's College, Malta, by the Rev. J. F. Dobson, has lately been received. The astronomy done at Stonyhurst is chiefly solar, but some stellar spectrographs have been taken, the results of which will doubtless be communicated to the Astronomical Society in due time. The drawings of spots made in 1900 number 157 on as many days, which is fewer than usual, as some alterations to the dome put the instrument out of use for a time. The mean spotted disc-area deduced from these drawings is 0·55 against 0·74 in 1899 (the unit being $\frac{1}{5000}$th of the visible disc). Father Sidgreaves says:—"It is not yet clear whether we have arrived at or passed the minimum epoch, and there is the same uncertainty about the maximum epoch of magnetic disturbances. Taking the extreme range of the Declination magnet on each day as a measure of the magnetic disturbance on the day, the mean of these for the year is 9'·7 against 12'·9 for the preceding year. But a small spot-group was sketched on September 2 and again on September 3 in latitude −25°, and this appearance in high latitude may be the forerunner of the expected revival of solar surface activity."

Two researches made at Stonyhurst during the year, one on the comparison between individual sun-spots and earth magnetic storms, the other on the duration of sun-spot disturbance, have already been noticed in these pages *.

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* See Reports of R. A. S. Meetings 1900 June and 1901 January.

PUBLICATIONS.

The Norwegian North Polar Expedition, 1893–1896: Vol. II. Scientific Results*.—This work has already been briefly referred to in these pages (*vide* Oxford Note-Book, 1901 February), but this fine large volume with which we have been favoured demands a fuller description, although the work scarcely lends itself to anything in the form of criticism.

It seems that Dr. Nansen, before he started on his well-known expedition, consulted various authorities on the scientific work that he would be able to do; he asked Prof. Geelmuyden of Christiania about astronomy, Dr. Mohn and Prof. Neumayer about earth-magnetism, Prof. Schiotz and Colonel Von Sterneck about gravity experiments; and these gentlemen, evidently realizing the importance of the occasion, gave their best advice, and each took care that the expedition was equipped in the best possible way for research in his particular science. Moreover, Dr. Nansen took an able officer, Lieut. (now Captain) Scott-Hansen, who, as is shown by the book, is a skilful and energetic observer, and the volume now before us contains in three sections the results of observations made chiefly by him, compiled respectively by Prof. Geelmuyden, Mr. A. S. Steen of Christiania, and Prof. Schiotz.

The astronomical results are by their nature not of particular general interest, since they were made mainly for determining the position of the party or for finding azimuth necessary for the magnetic observations. The observation of Johansen and Nansen on 1895 April 7 will be read with feeling of interest, for under that date there is set down "Meridian altitude of Sun, L. L. 10° 27″"; and since the declination of the Sun's lower limb on April 7 is 6° 35′, this indicates that the observers were well within four degrees of the pole. Truly a unique observation. The observations throughout are given with much clearness of detail, and the book may be recommended as a collection of arithmetical examples for the use of students who are learning the theory and formulæ for finding latitude and local time by sextant and altazimuth. Moreover, charts are added to the book, deduced from these observations, showing the track of the 'Fram' and the course of the sledge-journeys, which supply answers to the examples.

The magnetic observations are of more general interest, in fact it is scarcely too much to say that from a purely scientific point of view a series of determinations of the magnetic elements in the neighbourhood of the North Pole is quite the most important work that could be done by a polar expedition—first because of their rarity, and secondly because they *are near* the pole, where changes in direction of the magnet would be large at comparatively near stations. The observations were made by Lieut. Scott-Hansen, and during the 33 months from 1893 October to 1896 July he made determination of the magnetic elements on 194 days—first in a tent on the ice, then in a snow-hut, and afterwards in a house made of blocks of ice, always at such a distance from the ship that the iron in her could not be supposed to have any effect on the observations; but the observer had to make experiments to determine the effect of a revolver in close proximity—the presence of bears making the revolver a necessity. The result is a series of determinations of magnetic declination, horizontal force, and dip at various places. Dr. Schmidt, of Gotha, has computed the theoretical values of these elements for the same places, and compared the observed and calculated, which is a system that he has strongly advocated in all cases, and of which the desirability is obvious. The table Obs. — Cal., given in this book, cannot but be of the utmost value for the improvement of the magnetic theory; it may be summarized here by saying that the observed declination agrees with the computed in longitude 130° E., but is smaller going eastward from this, and larger going westward. There is one very remarkable observation: on 1894, November 24, the declination appeared to change by 26° in about fifteen minutes. The observer notes that the sky was overcast, but assumes that there must have been a considerable amount of aurora borealis. In longitude 12° E., latitude 83°, the observed declination was about 8° smaller than the computed. The observed horizontal force was smaller than the computed throughout by about one-third of the whole amount, and the observations of dip were throughout in excess by about 45′ in the mean. The observations of magnetic declination are exhibited in the same charts as the voyage and journey, which conveniently show the main results.

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* By Fridtjof Nansen. Longmans, Green, & Co., London, 1901. 30s. net.

The gravity experiments with pendulums are of the same value as the magnetic, since they add to our knowledge of a region peculiarly inaccessible. Half-second invariable pendulums, with the coincidence apparatus of the kind designed by Col. Von Sterneck, were procured for the expedition, and these also were put in the charge of Lieut. Scott-Hansen, who made determinations of the relative value of gravity. The accuracy of these may be questioned by some, since they were of necessity made sometimes in the saloon of the ship, and the stability of the support may be thought not to have been sufficient; but Lieut. Scott-Hansen has satisfied himself that this source of error has little, if any, effect. The results of these observations have been drawn up for press by Prof. O. E. Schiotz, who adds some interesting remarks. He says:—

> Nansen's expedition has furnished the first answer to the question as to what are the facts with regard to the force of gravity over great ocean depths. The observations show that the gravity may be regarded as normal over the polar basin; and as it is not probable that this is a peculiarity of the Polar Sea, we are led to the assumption that the force of gravity is normal all over the great oceans. The increased attraction observed on oceanic islands must therefore only be due to the local attraction of the heaped-up masses at the bottom of the ocean that form the islands.

NOTES.

Comet Notes.—The first comet discovered in the 20th century is a very brilliant one, though not as yet visible in northern latitudes. It was discovered on April 23 by Mr. Halls, of Queenstown, Cape Colony, and is stated to be a remarkable object with a triple tail 10° in length. A telegram from Sir David Gill gives the following position:—

G.M.T.	R.A.	N.Dec.
Ap. $24^d\ 17^h\ 5^m$	$1^h\ 30^m\ 4^s$	$3°\ 27'$

It thus rose at Capetown some 2 hours before sunrise. It was stated to be moving rapidly towards the Sun, so that it may pass to the north of it and become visible in these latitudes.

A. C. D. C.

Minor Planet Notes.—Another planet, GI, of magnitude 11·9, has been discovered by Herr Carnera, one of Dr. Wolf's assistants, at Heidelberg, on April 21.

Two planets, both discovered by Dr. Wolf in 1899, have been named at his request by Dr. Palisa, (447) Valentine, (443) Natalie.

Some further suggestions regarding the variability of Eros have been published in *Ast. Nach.*

It is suggested that two minor planets may have collided and adhered to one another, forming a dumb-bell-shaped object. This would clearly explain the light-variations in much the same way as M. André's hypothesis, except that the relative orbit would necessarily be circular, the two bodies being rigidly connected, so that the difference in period of the two waves would not be explained. It is further suggested that this hypothesis of collision would explain the unique orbit of Eros; but this suggestion does not seem quite sound. For both planets would have been travelling round the Sun in the same direction before the collision. Hence the more slowly moving one would have its speed and its mean distance increased by the collision, and so must have had a mean distance somewhat smaller than that of Eros before the collision.

Herr Ristenpart (*Ast. Nach.* 3705) does not favour M. André's hypothesis, and even describes it as "impossible"; but his grounds for this criticism do not seem sufficient; indeed he does not seem to understand that André explains part of the variation by the spheroidal form of the components, and part by mutual occultation.

Herr Ristenpart explains the light-curve by two bright and two dark regions on the planet, whose longitudes are

1st dark spot	0°	2nd dark spot	195°
1st bright spot	91	2nd bright spot	285

This is, of course, a very obvious suggestion, but the albedo of the bright spots must be at least six times that of the dark ones to explain so large a variation of light. It is difficult to imagine such a difference of albedo on so small a planet, on which we cannot imagine anything of the nature of clouds or snow. A. C. D. C.

OBITUARY.—We have to announce with regret the death of Dr. Henry A. Rowland, Professor of Physics in the John Hopkins University, Baltimore. Prof. Rowland's name will be associated by most persons with the word "grating," and our readers may remember a graphic account of a visit to his laboratory and the ruling of gratings written by our Oxford contributor in 1896 December. Possibly Prof. Rowland's most valuable personal contribution to astronomy was his photographic map of the solar spectrum, made with a concave grating of 21½ feet radius and 6 inches diameter, in which the definition is so good that 1474, b_3 and b_4 are distinctly shown as double lines. Prof. Rowland was a member of numerous learned bodies on this side of the Atlantic. He was a Foreign Member of the Royal Society, a Fellow of the Cambridge Philosophical Society, an Officer of the French Legion of Honour, and an Associate of the Royal Astronomical Society. He was in his fifty-third year at the time of his death.

THE COMET.—There has been no authoritative telegram of the position of the comet seen in the southern hemisphere on April 24 since Sir David Gill's telegram on April 26, from which the position given in our Comet Notes was derived; but the daily papers of to-day (April 30) state that the comet was seen at Yerkes on the morning of April 27, 15° north of the Sun, and from the note it appears to have had about the same R.A. If this is confirmed the apparent motion of the comet is remarkable, but the statement leaves us in doubt as to whether the comet has, between the dates above mentioned, passed round the Sun on its eastern side, and is now going westward, or whether it has described an orbit concave to the Sun on the western side and is now going eastward. If the latter be the case, the comet ought to be visible here in the evening. It is difficult to understand why Yerkes did not telegraph the information at once to Kiel.

NOVA PERSEI.—The new star in Perseus continues to diminish in brightness, but exhibits some fluctuations, which have led to the suggestion, based at first on the observations of March 19, 22, and 25, that the star was variable, with a regular period. A later suggestion has been made that the period is increasing; but if this be so, the word "period" somehow seems to lose its meaning. Many observations have been published in the *Ast. Nach.* and other places, and these have been tabulated under their several dates, and the mean taken for each day. These means (to tenths of a magnitude) are given in the following table. No attempt has been made to give the time to which they apply, although there is some evidence of variation during an evening; but little error is probably made by taking the values as applying to about 10 o'clock in the evening at Greenwich. The suffix figures show the number of observations which go to make up the mean. It will be seen that the record for April is at present somewhat deficient.

		m.			m.			m.
Feb.	21....	$1{\cdot}5_1$	March	15....	..	April	6....	..
	22....	$0{\cdot}5_3$		16....	$4{\cdot}0_3$		7....	..
	23....	$0{\cdot}2_6$		17....	$3{\cdot}7_1$		8....	$4{\cdot}7_1$
	24....	$0{\cdot}4_5$		18....	$3{\cdot}7_2$		9....	$4{\cdot}4_1$
	25....	$1{\cdot}1_4$		19....	$5{\cdot}2_2$		10....	$5{\cdot}3_2$
	26....	$1{\cdot}4_5$		20....	$3{\cdot}6_3$		11....	..
	27....	$1{\cdot}6_5$		21....	$3{\cdot}9_1$		12....	$5{\cdot}5_2$
	28....	$1{\cdot}7_6$		22....	$5{\cdot}2_4$		13....	..
March	1....	$1{\cdot}9_7$		23....	$3{\cdot}8_1$		14....	..
	2....	$2{\cdot}1_3$		24....	$4{\cdot}3_3$		15....	..
	3....	$2{\cdot}2_4$		25....	$5{\cdot}3_3$		16....	$5{\cdot}1_1$
	4....	$2{\cdot}5_3$		26....	$4{\cdot}1_2$		17....	..
	5....	$2{\cdot}6_7$		27....	$4{\cdot}1_3$		18....	..
	6....	$2{\cdot}9_8$		28....	$4{\cdot}6_2$		19....	..
	7....	$3{\cdot}1_4$		29....	$3{\cdot}8_1$		20....	$5{\cdot}4_1$
	8....	$3{\cdot}1_3$		30....	$4{\cdot}4_1$		21....	$6{\cdot}3_1$
	9....	$3{\cdot}5_3$		31....	$4{\cdot}5_2$		22....	$6{\cdot}0_2$
	10....	$3{\cdot}3_3$	April	1....	$4{\cdot}5_2$		23....	$4{\cdot}4_1$
	11....	$3{\cdot}5_3$		2....	..		24....	..
	12....	$3{\cdot}3_4$		3....	..		25....	$5{\cdot}4_1$
	13....	$3{\cdot}7_3$		4....	$4{\cdot}3_2$		29....	$6{\cdot}3_1$
	14....	$3{\cdot}7_3$		5....	$4{\cdot}7_1$			

No great reliance must be placed on the accuracy of the later figures, but the diminution of light on the 21st and 22nd, and the increase on the 23rd, is given on good authority.

ON THE HEAT-RADIATION OF ARCTURUS, VEGA, JUPITER, AND SATURN.—In the *Astrophysical Journal*, 1901 March, Prof. E. F. Nichols gives an account of his experiments made in the heliostat room of the Yerkes Observatory during the summers of 1898 and 1900.

Dr. Huggins, in 1869, and Mr. E. J. Stone, in 1870, made similar experiments with a thermopile and galvanometer, and obtained some heat effects which were undoubtedly due to some accidental cause. The failure attending these experiments led to adopting more delicate apparatus by Professor C. V. Boys in 1888.

In Boys's apparatus the receiving surface of a radiomicrometer suspension was mounted in the focus of a 16-inch reflecting telescope of 67·8 inches focal length. The most delicate suspension employed presented a blackened receiving-surface of 4 square millimetres, and was hung from a quartz fibre of $\frac{1}{5750}$ inch diameter. The period of vibration was 10 seconds, and the sensitiveness was such that without mirrors or lenses for concentration, the heat from a candle at a distance of 60 inches gave a deflection of 60 mm. Prof. Boys was thus able to detect $\frac{1}{150{,}000}$ of the heat sent by the full Moon on to the mirror, but he could detect no certain evidence of heat from Venus, Jupiter, Saturn, Arcturus, Vega, Capella, nor Altair.

In the present experiments the radiometer used was at least twelve times as sensitive as that employed by Prof. Boys, and as compared with the bolometer or thermopile has the advantages that (1) it is uninfluenced by magnetic and thermo-electric disturbances, (2) it is free from disturbances due to convection-currents. But it is not so portable as the thermopile, and the rays are subject to selective absorption.

The plan of the apparatus briefly is as follows:—A beam from the star was thrown into the room from a heliostat, and fell upon a two-foot concave mirror, the converging rays from which were caught by a 45° flat, and directed into the radiometer case through a fluorite window, the focal point lying in the plane of the vanes. The deflections of the radiometer were read by means of a telescope. The final results of Prof. Nichols's observations are thus briefly summed up, after making all necessary corrections. If the relative intensities be expressed in 10^{-8} metre candle, after the zenith-reductions have been made, then

Vega = 0·51
Arcturus = 1·14
Jupiter = 2·38
Saturn = 0·37

Thus the thermal intensity of

Vega : Arcturus : Jupiter : Saturn :: 1 : 2·2 : 4·7 : 0·74.

The zenith photometric intensities are

Vega : Arcturus : Jupiter :: 1 : 1 : 7·8.

The ratio of the total radiation of Arcturus to that of Vega being greater than 2 to 1, while they are of nearly equal photometric magnitude, indicates a proportionately more intense infrared spectrum for the former than for the latter star. The greater intensity of Arcturus may be accounted for in two ways. The photosphere of Arcturus may be at a lower temperature than that of Vega, but the star may be of sufficiently greater angular diameter, as seen from the Earth, to equal Vega in light intensity and surpass it in total radiation. This would be without doubt the first explanation to present itself. Recently, however, Sir William and Lady Huggins have brought forward evidence to show that the photospheres of solar-type stars are actually hotter than in stars of the first type, and that the colour-difference is due to the absorption of the stellar atmosphere, which in solar stars is denser and more developed. If this theory be accepted, it will not be necessary to assume a greater angular diameter for Arcturus than for Vega to explain the present results.

The thermal intensity of Arcturus to Jupiter is 1 to 2·2, while the light is 1 to 7·8, and may be explained in three ways: an infra-red spectrum of great extent and intensity for Arcturus; a comparatively low temperature of the outer envelope of Jupiter; or a strongly-selective albedo for Jupiter in the infra-red. That Jupiter emits no light-rays is probable from the fact that Prof. Barnard was unable to follow any of the satellites into the planet's shadow, even with the light-gathering power of the 36-inch Lick telescope.

THE SOLAR ECLIPSE.—Prof. D. P. Todd wrote to us from Port Said, on his way to Sumatra, supplementing the information we published last month by saying that he has taken, besides the mirror and occulter, a double polariscope and an automatic camera which was built for the Japan eclipse in 1896, and took 83 pictures of the corona last May in Tripoli. It is arranged on this occasion to take 150 pictures during totality, and Prof. Todd adds that it could easily be arranged to take 500 on glass during the 6 minutes, if there were any object in doing so. As to the occulting shutter, he seems doubtful whether this will be a success, as he suspects that the figure of the occulting vane must be worked out independently for every new eclipse, and that some of the data requisite for the most satisfactory figure are not available until the eclipse is over.

PERSONAL EQUATION.—The subject of Mr. Bryant's paper read at the R. A. S. is one on which a great deal has been written, by Pape, Dunkin, Wolf, Safford, and others, but somehow we seem to get no nearer to a satisfactory explanation of this personal peculiarity. Perhaps the chief question to be decided in the matter is, "Do we see before we hear? or *vice versâ*? This, as M. Wolf said in the paper to which the Astronomer Royal referred in his speech, is not a physiological question relating to the time of transmission of force along nerves, but rather a psychological one. M. Wolf put it that there may be a *dead time* in the brain, between seeing and hearing two simultaneous phenomena. If this is so, an observer who observes chronographically, as the Astronomer Royal described, would record early, and when observing eye-and-ear would also record early, so that his equation in the two methods would be the same, both being equal to the *dead time*, and this does not account for the difference which is generally found. From experiments with his personal equation machine M. Wolf satisfied himself that the interval did not exist. His memoir, which is in Tome viii. of the 'Annals of the Paris Observatory,' is descriptive of his machine and his experiments, and he comes to the conclusion that though such an instrument is a useful thing for the instruction of tyros it is a bad thing for practised observers to experiment with, as it tends to make them alter their habit; and this also may be the effect of making or reading investigations on personality. The closer an observer can bring himself to resemble a machine the better his observations will be. If a clock has a steady rate (again we paraphrase M. Wolf) it is not advisable the alter the pendulum, even though the rate be large.

ANOMALOUS OCCULTATIONS.—Another paper read at the R. A. S. revived an already well-discussed subject. Mr. Innes promises to make a collection of the cases when stars occulted by the Moon appeared to "hang on the limb." He is no doubt familiar with the very complete collection of these up to date about 1860 in a memoir presented to the Society by Sir George Airy (the collection was actually made by Mr. William Ellis), so that if he continues the research from that date he will be doing excellent work. The most remarkable instance in Sir George Airy's memoirs is one in which he seems to have full confidence, and is an observation made, oddly enough, by a namesake of Mr. Innes, when Aldebaran was seen to hang on the limb for 5 or 6 seconds. If the hypothesis now revived has any foundation in fact, Aldebaran should be a spectroscopic double.

RED RAIN.—This meteorological phenomenon, like the astronomical "blue moon," is thought to be so unusual that the phrases are used as synonyms for the improbable. But they have been looking on the rain when it is red in various places lately, in Sicily and Italy and Austria. Prof. Rucker, who has chanced to be in Sicily lately, and others have investigated the matter and find almost without doubt that this is really due to desert sand carried over from the Sahara and deposited with or without rain. Prof. Rucker, from a rough measurement of the fall on the marble-topped tables of his hotel on March 20, computed that the dust fell at the rate of about 5½ tons per square mile (our information does not state the time taken for this quantity to fall).

Curiously, at the same time another cause for a phenomenon under the same name has been investigated nearer home. Dr. Mill at Camden Square noticed that the water in the evaporation-tank was of a red colour, and on stirring the water and taking out some in a glass the presence of minute bright red points just visible to the eye was seen. On these being submitted to microscopical examination it was found that they were specimens of a minute moving water-plant—*Sphærella pluvialis*—which doubtless was the cause of some of the reputed "rains of blood" recorded in history. Dr. Mill states that the tank and rain-gauges at Camden Square have not caught any red rain such as that which fell in Southern Europe.

SYMONS'S METEOROLOGICAL MAGAZINE, from which we learned the above particulars, is now under the able editorship of Dr. Hugh Robert Mill, who is introducing new and more popular features into the pages of our contemporary. The current number contains several pretty little landscape pictures—reproductions of the slides which illustrated Dr. Mill's lectures at the Meteorological Society on "Climate." The same number calls attention to a typographical error, which stated that the monthly rainfall at a certain place in the Lake District was 9 *feet* 2 inches, and has been reproduced with serious comment in a usually well-informed paper by a person who ought to have known better.

THE WEATHER IN APRIL.—Variable and unsettled weather marked the opening of the month, the mean daily temperatures until the 9th fluctuating to small extent on either side of the average, the warm days, however, slightly preponderating, and the mean for the period being 0°·5 above the average value. On the mornings of April 2, 5, and 6, the thermometer fell below the freezing-point, the lowest recorded temperature being 30°·3 on the 2nd. At times it was fairly bright and sunny, the sunshine amounting to 50·7 hours, which is equivalent to 43 per cent. of the possible duration, the record on three days, the 1st, 8th, and 9th, yielding 30 hours out of the total amount. The prevalent wind was S.W., and rain fell on 5 days to the amount of 0·90 inch. A cold, wet period, with strong westerly and north-westerly winds, prevailed during the succeeding 8 days, the mean temperature being 3° below the average, and the sunshine 21 per cent. of the possible amount. Rain fell on 7 days amounting to 0·89 inch, and the barometer was low from the 10th to the 15th. A brilliantly clear and sunny period set in on the 18th, and it was remarkably warm, with south-easterly winds, until the 25th, the mean temperature being 8° in excess of the average and the highest temperature reaching 76°·6 on the 23rd. This is a higher temperature than any experienced in April since 1893, April 20, when a temperature of 80° was recorded. Bright sunshine from the 19th to the 26th amounted to 96 hours, being equivalent to 84 per cent. of the possible duration, whilst on the 21st and 26th the amount on each day exceeded 92 per cent. No rain fell, and the air was abnormally dry. After the 27th it become colder and cloudier, the wind shifting to N.E. and sunshine falling off in amount, and there appeared then to be no immediate prospect of decided change.

THE NATIONAL ANTARCTIC EXPEDITION.—The object of this expedition, which is shortly to set out in the ship 'Discovery,' is not, it is definitely stated, to reach the South Pole, but to investigate the scientific problems of the Antarctic regions, and naturally there is an astronomer among the members of the expedition, although, as we have pointed out in the notice of Nansen's book, there is nothing astronomical to be learnt specially in polar regions. The object of primary importance is terrestrial magnetism, and it was for this subject mainly that the Treasury was appealed to for funds, and for its needs it was thought advisable to provide a new vessel. Mr. William Shackleton, late of the Solar Physics Laboratory, is to be the astronomer and physicist of the party, and will hence take a large share in the magnetic work. Mr. Shackleton will be remembered as the successful observer of the total solar eclipse of 1896. We wish him *bon voyage* and equal success in this enterprise.

U.S. NAVAL OBSERVATORY.—From an American magazine we learn that the United States "Congress" did not find time to deal fully with the affairs of the Washington Observatory, but an amendment to the naval appropriation bill was passed ratifying the present state of affairs—*i. e.*, the existence of a Board of Visitors, a naval officer as Superintendent, and an Astronomical Director, only "until further legislation by Congress." Meanwhile, our information goes on, the unfortunate state of affairs at the Observatory is emphasized by the fact that the Superintendent has placed the Astronomical Director under arrest for trial by court martial, owing, it is alleged, to his having used influence against the Superintendent.

THE announcement made by the President at the April meeting of the Society must have been heard with feelings of gratification by astronomers. The representatives of the Royal Society naturally had personal audience with His Majesty, but of the other learned Societies, one was chosen to represent them all, and that one was the Royal Astronomical.

WE are informed that the business of the late Alvan Clark and Sons has been incorporated under the title of "The Alvan Clark and Sons Corporation, Telescope Makers, Cambridgeport, Massachusetts."

Visitation-Day at Greenwich will be on Saturday, June 1. The Visitors will dine afterwards at the Criterion Restaurant, Piccadilly Circus.

The next Meeting of the Royal Astronomical Society will be on Friday, May 10; of the British Astronomical Association on Wednesday, May 29.

From an Oxford Note-Book.

The first report of Mr. F. L. O. Wadsworth as Director of the Allegheny Observatory, which is accompanied by a beautiful plan and picture of the new buildings, promises well for the future. Mr. Wadsworth has found time, although he must be very busy with his new duties, to devise and to carry out a satisfactory method of using curved plates in photography, whereby the available field of a doublet lens is increased from 8° to 30°! Corresponding to the three different foci (primary and secondary focal lines and circle of least confusion) there appear to be three distinct fields of regular but different curvature. The best defined image was found in the primary focal field, which was accordingly selected for use.

He hopes to get fields of 45° in diameter, with which the heavens could be photographed on very few plates. Some fifteen years ago curved plates were tried at Greenwich, with indifferent success. It is satisfactory to hear that the experiment has been more fortunate on this occasion: and if Mr. Wadsworth can see his way to measuring the plates or copying them on flat plates he will complete a noteworthy step in advance.

May I plead with him for a little more accuracy in his numerical statements? He says that with a field 45° in diameter "the entire visible sky could be photographed on 16 plates." Should not this be 24 at least? The sky is divisible into 8 similar right-angled spherical triangles, for each of which at least 3 plates are required. Then take the following sentence:—"With the instruments used by the International Astrophotographic Association, about 1400 plates are required to cover the entire sky, and the work begun in 1886 at 17 different observatories is still far from finished." Passing over the odd name for the co-operating observatories, the number of plates should be 11,000 (accurately 22,154 to cover the sky twice), and the work was not begun in 1886, as the Conference was first called in 1887 and only then was the type of instrument determined upon; some of the instruments took a long time to make, and some of the 18 (not 17) observatories have not yet started work. These facts should be in many reference-books lying ready to the hand of an astronomer, and it is just as well to have them right in the official report of an astronomer. There is another point which may not be generally realized and which it may be well to mention: viz., that the delay in completing the work is to a large extent deliberate. It seems better to take the plates no faster than they can be dealt with. At Oxford we could certainly have taken all the plates in one or two years if necessary; but we could not have *measured* them; and sooner than leave many of them lying idle on the shelves, exposed to injury and possible fading, it seems better to take the plates at the rate they can be measured at. Similarly with those which are not to be measured, but reproduced: the reproduction necessarily goes on slowly, and it is better to have a fresh plate than an old one. We may, however, sooner or later decide to adopt freely the toning-process suggested by Sir W. Crookes; and if this is successful, there will not be the same objection to keeping plates for a time.

THE experiments of Prof. E. F. Nichols * on the Heat Radiation of the stars are most interesting. The radio-micrometer in the able hands of Mr. C. V. Boys failed to show any indications of heat from the stars, though it would have detected a candle two miles off. Using a radiometer form of instrument and a heliostat or coelostat to reflect the star-light on to a 2-foot mirror, Prof. Nichols has got indications from Vega, Arcturus, Jupiter, and Saturn; the actual values (on which, however, he is careful not to lay too much stress) being 0·5, 1·1, 2·4, 0·4 of 10^{-8} metre-candle, corrected to the zenith. He thinks a good deal might be done with a 5-foot mirror, mounted in some coudé manner; and urges that, considering the possibilities of such an instrument in other ways (photography of faint nebulæ, and of star-spectra, &c.), "the addition of such an instrument to the equipment of one of our leading observatories would provide the means for solving a greater number of outstanding problems than the addition of any other single instrument which it is now possible to build." The reflector has always been regarded as an essentially English instrument, and it is pleasant to hear it appreciated, even in possibility.

WITH regard to the difficult question of duplicate publication, I would respectfully ask the Editor of the *Astrophysical Journal* what is his rule with regard to the many reprints of papers which it is of such immense convenience to find in his Journal? He has struck out a line with which I have the heartiest sympathy—that of collecting into one channel most or all of the papers connected with Astrophysics; it is a mere matter of detail I wish to enquire about—what is the rule of acknowledging the sources of some of the papers? The other day I quoted from a paper on the siderostat by M. Cornu, which I read in the *Astrophysical Journal* for March 1900; I had occasion to mention a slight numerical error, and subsequently received from the author a copy of the *original* paper (in French) from the *Bulletin Astronomique* of February 1900, where the error in question did not occur. But there is no indication in the *Journal* of the source of the paper. In commencing work on the International Scientific Catalogue, one of the questions raised has been the treatment of papers which occur in several journals in essentially the same form. It will be an obvious convenience to those using the Catalogue to know at once that these papers are the same; they can then refer to the alternative nearest at hand and neglect the others. And if reprints are accredited in a conspicuous manner to their sources, the indexer will be able without much trouble to give such information as this in the Catalogue. Otherwise he may have to read both papers carefully: or to avoid risks may have to index them as separate papers. I would ask the consideration of our American friends for such points as these. Their practice as regards printing papers is a little different from ours: they print the same paper in several journals, so that it may be widely read, whereas in Europe we have made it a point *not* to reprint.

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* *Astrophys. Journal*, March 1901.

THE European practice of not printing in more than one journal arises, no doubt, from reasons of economy. It is considered rather wasteful of the slender resources available for scientific publication to duplicate the printing of the same paper. But a method of procedure has recently been adopted by the Royal Society and Royal Astronomical Society which is free from this objection, and has all the advantages secured by the American method, which are undoubted. An arrangement has been made between the two Societies whereby, when papers of astronomical interest are read to the Royal Society and printed by them, additional copies shall be struck off the same type for circulation with the *Monthly Notices* or *Memoirs.* The additional cost is trifling, and these papers (often very important ones, and because of that importance communicated to the Royal Society), which formerly only reached Fellows of the Royal Society in the ordinary course, will now reach all those who get the *Monthly Notices.* This is a procedure which may well be found advantageous in other cases. It means that the two sets of publications must be sensibly of the same size; and if this is not so one of them must be altered. Thus the size of the *Memoirs of the R. A. S.* has been altered to be the same as that of the *Philosophical Transactions*, though fortunately no alteration is needed in the size of the *Monthly Notices*. But this alteration is itself a step in the right direction. Greater uniformity in size of publications has long been felt to be desirable, and several Committees have sat on the subject without effecting anything much. The reason for this ill-success has been partly that there was little to be gained by those called upon to make changes. A positive gain of the kind above indicated may make all the difference in such cases.

ANSWERS to examination questions:—

Aberration is a reflection or deviation from a straight line.

Thus the Sun's rays fall on the Earth, and thence are reflected to the Moon, causing the appearance known as the "old Moon in the new Moon's arms."

A ray of light sent through a prism is also deviated from its original course.

There is also aberration of Sound, as witness the echo.

The *Harvest Moon* is that Moon which appears to be full for several nights together. The reason we do not get the full Moon for several nights together in the spring is because the Moon is then between the Sun and the Earth; therefore the lit-up portion of the Moon is hid from the Earth.

Precession.—As the Earth rotates on its axis the ends of the axis gradually change places; the North Pole very slowly gets to the position of the South Pole, and the South Pole becomes the North Pole.

A NEW SOUTH WALES astronomer sends cheery news of the doings of the N.S.W. Branch of the B. A. A., and appends the following extract from the speech of a well-known politician in proposing the toast of "The Ladies":—"The Moon in her inconsequentiality may disappear from the heavens; the stars may burn out and become blind — stone blind; the Sun may be shattered into fragments and fall towards the Earth — but Woman, Lovely Woman will remain with us." A critical mind will notice that this compliment, high-flown though it is, only assigns to the subject of it the second place as regards permanence, for it is pre-supposed in the last two words that there is no destruction of "us."

SOMETIMES a practical illustration of the number of units in a million is of interest. On the arrival of a new Greenwich volume the other day, I had the curiosity to make a rough estimate of the number of figures contained in it. Recent volumes seem to contain, in addition to nearly 200 pages of introductory matter, reports, &c., about 1000 pages, with an average of 2000 figures on each, so that the whole volume contains about two million figures. The size varies somewhat; the volume for 1893 contains 1500 pages and three million figures.

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EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 May 10.

Dr. J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., and W. G. Thackeray (*vice* F. W. Dyson).

The Minutes of the previous Meeting were read and confirmed.

The President. I have to announce that I was asked by the Council to approach His Majesty the King with the request that he would become the Patron of the Society in succession to His Majesty William IV. and Her Majesty Queen Victoria, and I will now read you the following letter which I have received:—

> Sir,—
>
> I have submitted your letter of the 15th instant to the King, and I am now commanded by His Majesty to say that he is pleased to accede to the request contained therein to become Patron of the Royal Astronomical Society.
>
> I have the honour to be, Sir,
> Your obedient Servant,
> (Signed) D. M. Probyn, General,
> *Keeper of H.M. Privy Purse.*

Mr. Whittaker. 97 presents have been received since the last Meeting. Among these may be specially mentioned the gift by Mrs. Cross, widow of the late Canon Cross, formerly a Fellow of the Society, of the instruments belonging to his observatory in Lincolnshire, namely, a Newtonian telescope with 18-inch reflector equatorially mounted, a transit instrument, and several others. From the Netherlands Government we have received the 6th and last volume of Prof. Oudeman's work on the triangulation of Java; and from Prof. Laussedat we have received a copy of his work 'Recherches sur les Instruments, les Méthodes et le Dessin topographique.'

A vote of thanks was accorded to the donors of the presents.

Mr. A. R. Hinks exhibited and described the Cambridge instrument for measuring celestial photographs. He said that, three years ago, Sir Robert Ball had entrusted him with the preparation of general plans of the machine, which were sent to the Cambridge Scientific Instrument Company, and on these Mr. Horace Darwin based the design. It was impossible to express too warmly the thanks that were due to Mr. Darwin for the care and thought he had given to every detail. At Cambridge a réseau impressed upon the plates was always used, and the machine was of the type which gave the position of the star-image relatively to the réseau lines. There was an objection to a machine in which a screw was the sole measuring agent because the screw quickly wore out, and a scale for measuring, such as was used at Greenwich and Oxford for the Astrographic Chart, was not exact enough for parallax work, and the idea was to combine the two. [A mechanical slide was exhibited on the screen to show the method of measuring the distance of a star from a réseau-line by means of a divided scale and micrometer screw.] A complete measure was made by turning a screw through a small fraction of a revolution, which saved the wear. The optical parts were made by Messrs. Zeiss, who had invented a new kind of reversing prism eyepiece. The screws were made by Messrs. Brown and Sharpe, of Philadelphia, and it was hard to detect any error in the screw. The one examined was perfect for at least 10 revolutions. The scale was almost equally perfect, and it seemed that this was a machine in which the errors could be treated as accidental. From discussing the results from a photographic plate, the probable error of a measure appeared to be about ·0004 of a réseau interval (3′). The error seemed to depend much more upon the plate and the telescope than upon the measuring-machine. Upon a plate where the star-images were never perfectly clear, the results are never quite good. Mr. Hinks was inclined to think that the Cambridge machine was a considerable success.

The Astronomer Royal. I should like to say a few words to express my great interest in this new machine which Mr. Hinks has brought before us. It is a question in which I am very much interested, and I am very much struck with the way in which Mr. Hinks has adapted the glass scale to more accurate measurement. The same difficulty confronted me at Greenwich in the measurement of Eros. We happen to have a filar micrometer there with wires, and I adopted an arrangement which is somewhat equivalent, though not so complete, as Mr. Hinks's. It may be a useful principle for other measures. It is simply this—that in order to avoid the great number of turns of the screw, a number of wires are put in which really take the place of some of the divisions in the scale. (Blackboard.) I am very much struck with the great ingenuity shown in this measuring-machine and the beautiful way in which the design seems to have been carried out.

Prof. Turner. I should like also to express my admiration for this machine, which I have had the opportunity of seeing at Cambridge and trying rather more thoroughly than it can be tried here. As an instance of the carefulness with which Mr. Hinks has gone into a great many of the details, I should like to lay stress upon the fact that the head of the screw is placed at a convenient distance for reading. People who work for hours together with a microscope and find their eyes becoming tired may not in some cases have realized how much of that is due, not to the work you are necessarily doing, but to the fact that you are continually changing the focus of the eye by leaning back to read the microscope. These matters may seem small ones to those who have not had experience of such work, but when measures are kept up for days and weeks together they are very important. I could comment favourably upon all the details, but that one struck me especially as worthy of note.

Prof. Rambaut. I should like to express my appreciation of this instrument, which seems to differ from many others. I can speak with some experience in this matter, and I feel I am in some respects particularly well qualified to appreciate the beauties of this instrument, having worked with one of the earlier types of micrometers made for the purpose of measuring astronomical photographs. I speak of an instrument made on the same lines as Prof. Pritchard's first instrument, in which none of these delicate means of getting over the difficulties had been introduced. I think the first inclination one has on seeing an instrument of this sort is to fall down and worship it, and when I saw it the other evening at the Royal Society, that is the feeling I had. Then a further degree of familiarity suggests the possibility of finding faults in it, and I looked about to see if any such fault could be found; but I must say I found great difficulty in doing so. There is one point I wish to ask Mr. Hinks about, and that is about the change in the temperature taking place at a different rate in the glass scale in the microscope and the glass plate to be measured—what effect will that have upon the measurement? I presume such a short portion of the screw is used that the temperature-effect upon it will be quite insensible, but there is a fundamental assumption that the scale and the plate will expand or contract equally with the temperature. There is just one other point which seems a very small matter, but it may become of importance. It struck me in looking into the eyepiece that the diamond-cut lines were in one or two places chipped off. I think it was mostly in the figures.

Mr. Hinks. The figures are bad, but the lines are quite perfect.

Dr. Rambaut. With a diamond-cut line I should imagine that the edges will sooner or later become a little jagged. This is, however, such a small point that, in view of the many beauties of this instrument, I do not wish to press it, but would like to end as I began, by expressing my admiration for the great ingenuity Mr. Hinks has shown in getting over all the various difficulties which arise in the construction of such an instrument.

Mr. Hinks. I am much obliged for the appreciation which has been shown of the instrument. Dr. Rambaut has mentioned two points—the effect of unequal expansion with temperature of the scale and the screw, and possible imperfections of the diamond cuts. As to the first, change of temperature alters the ratio of scale to screw; but this is inappreciable when only a small fraction of a turn is used, and it produces error of runs which can, however, be so easily adjusted that the run need never amount to more than a few units in the last place. As to the diamond cuts, the figures look bad, but they are only a tenth of a millimetre high, and the surface of the glass *will* break up under a curved cut. I have not noticed anything with regard to the straight cuts but that they are as perfect as they can possibly be. I forgot to mention just now the very ingenious device designed by Mr. Horace Darwin to obtain two motions of the scale at right angles, which is provided by two micrometer-screws at right angles abutting on a square block. I have a model of this here which can be passed round for examination.

The President. The Society have already expressed their appreciation of Mr. Hinks's instrument, and the Society will congratulate him upon its completion. We hope he will have his reward in the satisfaction he will find in using it for many years to come.

Dr. W. J. S. Lockyer then exhibited some slides, which should have been shown at the last Meeting of the Society, of the spectrum and magnitude of Nova Persei.

The Rev. W. Sidgreaves (presenting a fourth note on the spectrum of Nova Persei). This note on the spectrum of Nova Persei refers to certain well-marked changes in the spectrum, which for a time were periodic, as they recurred at the times of minimum general brightness of the star. These changes have been described in a previous note, and the changed spectrum has been called the minimum type spectrum. It has reappeared on several dates since our last meeting, and these are given in the paper, together with the dates on which the spectrum was found to be of the non-minimum type. The three-day period broke down on the 11th April, if not before; but it will probably be found that the minimum type spectrum has continued to follow the subsequent minimum epochs of the light curve.

It is now necessary to add that the so-called extension of the bright Hζ band does not appear to belong to hydrogen, and that the brightest of the blue bands cannot owe its prominence to contrast effect. Both bands seem to come into existence at these times, and disappear when the star is brighter. The band adjoining

Hζ on the spectrograph of March 28 has two well-defined greater intensities, closely agreeing in position with two strong cyanogen lines, and is called provisionally in the paper the CN band; and the bright blue band occupies the position of the space between two bright bands of the non-minimum type spectrum.

An addition to the characteristics of the minimum type spectrum appeared for the first time on April 11, when the intensities of the bands CN, Hϵ, and Hδ were in the inverse order of the photographic sensibility of the plate—the CN band being the strongest, and Hδ the weakest of the three; and this order of intensities was repeated on the 16th, 20th, 25th, and 26th.

The Rev. A. L. Cortie, presenting a paper on the "Visual Spectrum of Nova Persei," said that it was with some hesitancy that he brought forward his observations on account of the comparatively small instruments with which they had been made—a 5-inch object-glass and a Maclean eyepiece stellar spectroscope; he had no micrometer to make exact measurements of the positions of the lines. However, he had been able to identify them with sufficient accuracy by means of comparisons with the photographs taken by Father Sidgreaves and Professor Hale. The number of lines seen in such a small spectroscope was remarkable. On March 21st, no less than 16 were seen, from a line presumably due to helium at λ 668 to Hγ. The details of the observations were contained in the paper, and they would possibly serve to fill up breaks in the observations of other more fully-equipped observers. He had, however, been able to follow the fluctuations in the brilliancy of the several lines very well. On March 21st a bright band at λ 466 forced itself on notice, and it had remained conspicuous ever since. When the star was at a minimum, as on March 25th and March 28th, a remarkable shifting of the luminosity took place towards the yellow end of the spectrum, and on these dates the bright yellow line was quite strong. In the conclusions appended to the paper the varying comparative brightness of the lines Hα and Hβ was followed, as also of Hβ and the line near λ 500. At the end of April this last line had attained a supremacy of brilliancy over all the lines of spectrum. Finally the bright-line spectrum was very similar to that of the chromosphere, a point which struck observers also in the case of the spectrum of Nova Aurigæ.

Prof. Turner then described a paper by *Mr. F. A. Bellamy*, it being an additional note on the position of Nova Persei and a comparison of photographic magnitudes of neighbouring stars with those of Father Hagen's chart and catalogue. He said that the paper was more suitable for printing than for reading. Mr. Bellamy had compared with the Oxford measures Father Hagen's positions of the neighbouring stars. These were sufficiently good to identify the stars until they came to the faint ones. The number of faint stars was so great that positions given by Father Hagen (which were accurate to a second of time) were not good enough to identify them in all cases. During the examination other stars were found that had been omitted from the measures, indeed so many that as Mr. Bellamy went on re-examining the plate he almost thought stars were *coming up* on the plate. He did not know if that was photographically possible: they heard a good deal more about stars disappearing from plates, and perhaps that was the more likely. 16 more stars altogether were discovered. It was a subject that was worthy of attention.

Mr. Wickham (presenting a paper on "Further Observations of the New Star in Perseus made at the Radcliffe Observatory"). I regret that Dr. Rambaut has been obliged to leave the Meeting, but he has delegated to me the duty of placing before the Society some account of the observations on Nova Persei during the last month. The first slide, however, should have been shown at the previous Meeting, as it represents graphically the determinations of magnitude visually from the time we first saw the Nova at Oxford up to April 18. It shows that the minimum of April 6 apparently lasted during two whole nights, and the bluntness of the curve suggests that the actual minimum occurred in the interval between the times of observation and is considerably lower in magnitude. The line of maxima gradually lowers from March 20 to April 10. The paper now presented constitutes the work from April 10. The estimates in the earlier part were made for the most part with the naked eye, but recently, owing to the small altitude of the object, we have been compelled to resort to the telescope. For many years I have been an observer with the telescope with but little experience of naked-eye work, but I have been struck with the unexpected agreement between different observers when observing without instrumental aid. From the results obtained, this class of work seems to offer valuable opportunities. With regard to the observations of colour,—during the observations at Oxford we have been able to tell when the Nova was approaching maximum or minimum, for at maximum the star assumed an orange colour, whilst for minimum it changed to a deep red. It has been further noted that at minimum the star's image in the telescope seemed to be lustreless. During the strong twilight, on the background of the sky, the contrast was not so marked, but when the night has become fairly dark the image of the star has been noticed to be larger in area than the comparison-star; yet one felt that owing to its lack of lustre and the different quality of its light, one must put it down as a fainter magnitude than the comparison-star. Then the question arises, What is magnitude? Is it to be gauged by intensity of lustre, or by size of image? There are some notes in the paper showing that when the star was dropping into the haze near the horizon, different effects were produced. Sometimes the light of the star seemed to be rapidly absorbed, but at others it seemed to maintain its brightness.

The last observation included in the paper is that of May 8 by

Mr. Robinson with the 10-inch equatoreal, when he noted an absence of any marked colour. An interesting incident of the Oxford observations is the corroboration they afford of those just presented by Dr. Lockyer, and the fact that when at London cloud prevented observation, we at Oxford have been more fortunate, and it has thus been possible to dovetail these two sets of observations very neatly.

The Oxford work on the magnitude which forms the subject of the present paper is graphically represented on the slide now shown.

Mr. Whittaker showed a slide sent by Mr. Ledger, representing by a continuous curve the magnitude of Nova Persei, and compared it with the curves already shown by Mr. Wickham and Dr. Lockyer. He then described two papers by *Mr. Stanley Williams* and *Mr. M. C. Sharp*, forming continuations of their previous papers *, giving observations of the magnitude during the past month. They appeared to give results very well accordant with those already described by Dr. Lockyer.

Prof. Turner. Mr. Ledger's photograph shows that the variation in light begins very much earlier than appears from the other photographs.

Mr. Whittaker. I think the early fluctuations were exhibited to some extent in the first slide shown by Mr. Wickham.

Mr. Wickham. There is probably a good deal of personality coming in, which may make one curve undulate and the other not.

The President asked Father Sidgreaves to make a statement to the Meeting respecting Nova Persei.

The Rev. W. Sidgreaves. The communication I have to make, and which is not contained in my Notes on the Spectrum of the Nova, would be more interesting to the Fellows if I were at liberty to mention the authority on which it rests †. A suggestion was made to me in a private conversation at the R. S. Soirée on Wednesday, and it only occurred to me afterwards that it would be of considerable interest this evening here, regarding the cardinal difficulty in every suggested reading of the spectra of new stars—viz., that if the separation of the bright and dark hydrogen lines is to be explained on Doppler's principle, the source of the dark lines must always be moving at high velocity towards us; and consequently a general law is needed according to which we are not to expect any variation of this order of position in the spectrum.

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* With reference to Mr. M. C. Sharp's first paper on the magnitude of Nova Persei, which was read at the April Meeting of the R. A. S., Mr. Whittaker writes to say that in his account of the paper at the Meeting, as reported in our last issue, the use of the phrase "conjectural curve" may give rise to a wrong impression of the nature of Mr. Sharp's diagram. The diagram was a plotting out of Mr. Sharp's own observations, the points representing successive observations being joined by straight lines: there was therefore, properly speaking, no element of conjecture in the diagram itself, and the word "conjectural" must be understood as referring only to the conjectural values of the star's magnitude, in the intervals between Mr. Sharp's observations, which would be furnished by these lines on the assumption of uniform change of magnitude in the intervals.

† Permission for this arrived just too late for the meeting, owing to the absence of Lord Kelvin from London.

The suggestion, briefly stated, is this: granted a collision between two stars, the immediate result would be scattering of matter in all directions, with greatest velocities in directions nearly perpendicular to the line of relative motion of the two impinging bodies. These velocities of fragments may largely exceed that of the relative motion of the two bodies before impact *. The matter thus scattered would consist not only of the broken bits but also of their decomposed constituents. Ubiquitous hydrogen would be there to begin with, and much also would be liberated. A part of this gaseous mass would be raised to high temperature, with little associated kinetic energy; and a part would be urged outwards at high velocity, with little rise of temperature; and this, the cooler gas, would be the absorbing material. The absorber therefore would always be in motion towards us, and the radiant matter would remain at comparative rest. The application of Doppler's principle then becomes simple. The mass of hydrogen moving away from the white-hot centre would absorb shorter wave-lengths of the white radiation, and would *not* absorb the radiation of glowing hydrogen. The bright lines would therefore be little or nothing displaced on the continuous spectrum, and the dark lines would always be shifted to the side of greater refraction.

The simplicity of the explanation commends it to astrophysicists: it meets what has been to me the chief objection to the application of Doppler's principle to the shift of the lines; and it is in keeping with the subsequent thinning away of the dark lines of Nova Persei, which very constantly declined in strength and width, and finally vanished between the 21st and 22nd of March. But I do not see, at present, how it can help us over the second difficulty: the great breadth of the bright lines, some of which seemed to have lost nothing in width up to the last days of April. We may perhaps hope that the author of this interesting suggestion will at some future time give us his views on this also.

Prof. Turner (reading a paper by *Mr. H. C. Plummer* on the rotation of the field of a siderostat). I have already twice spoken to the Society on this subject—on the first occasion on my own behalf and on the last occasion behalf of Mr. Plummer. The subject seems to have attracted attention, and there have been other papers published by M. Cornu and by M. Lippmann. Mr. Plummer's paper seems to me to be a very excellent geometrical proof of the fundamental propositions. I am astonished to find how much simpler these can be put than I had found possible after spending some time on the matter, and I commend the paper as having gone to the root of the thing in a very elegant way.

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* [*Note by* LORD KELVIN.—We see illustrations of this on a small scale in the breaking of waste masses of cast iron by letting fall upon them a heavy block to act as hammer. Remembering our conversation at the Royal Society, I spoke on the subject to my friend Mr. Wickstead on the occasion of a visit to his engineering works on Saturday last in Leeds, and was shown by him a window which had been recently pierced by a fragment of iron, broken by the fall of a hammer through a height of only ten feet. The window was above the level from which the hammer had fallen, and its horizontal distance showed a trajectory which demonstrated a velocity of the fragment shot away very much greater than that of the hammer at the instant before impact.—K., May 17, 1901.]

Mr. Crommelin in response to the President spoke of the new comet. It has been tantalizing to us to hear such brilliant accounts of the new comet in the Southern Hemisphere and to have to wait for such a long time before we have known anything accurate about it. Even now we have only one report of any accuracy. The original announcement came from the Cape Observatory. In the daily papers there were two complicated statements. One was that there was a comet 15° north of the Sun, and then Mr. Chambers said that he had seen the tail of a comet in the morning of May 15. Then there came a telegram from Arequipa saying that a comet had been seen in the evening sky, so it could not be the one that Mr. Chambers had seen. Fifteen days after that we got an accurate place from the Cape. So that this was not promising material to work upon from which to get the orbit. I have sent the positions to Dr. Smart, and I have tried to get an orbit myself and our results agreed closely, but that does not show that the orbit is right, it only shows that it is right according to the material available. It seems to be a very interesting comet physically. Mr. Maunder telegraphs from Mauritius that the comet appeared to have three tails, and that a photograph of it has been obtained. If the comet is seen from this country, however, it is to be feared that its beauty may have faded considerably.

Mr. Turner. Might I express a hope that an ephemeris will be published?

Mr. Crommelin. I am going to attempt an ephemeris, but it will not be very accurate.

Mr. Whittaker then read a paper by *Mr. Franklin Adams*, who had made some observations of the setting Sun on 19th April, in the course of which he saw a number of emerald-coloured beads resembling Baily's Beads. Mr. Adams suggested that astronomers should observe the phenomenon of the green flash at sunrise and sunset as often as possible, especially East-Coast astronomers at sunrise.

The following papers were announced and partly read:—

John Tebbutt. "Results of Double-Star Measures made at Windsor, New South Wales, in the Years 1899 and 1900."

Rev. A. L. Cortie. "The Visual Spectrum of Nova Persei."

Rev. W. Sidgreaves. "The Spectrum of Nova Persei, Note 4."

F A. Bellamy. "Additional Note on the Position of Nova

Persei, and a Comparison of Photographic Magnitudes of Neighbouring Stars with those of Father Hagen's Chart and Catalogue."

A. R. Hinks. "The Cambridge Machine for Measuring Celestial Photographs."

Radcliffe Observatory, Oxford. "Further Observations of the New Star in Perseus."

J. Franklin Adams. "The Green Flash at Sunset."

A. Stanley Williams. "Further Observations of the New Star in Perseus."

M. C. Sharp. "Further Observations of Nova Persei."

Royal Observatory, Greenwich. "Results of Micrometric Measures of Double Stars made with the 28-inch Refractor in the Year 1900."

H. C. Plummer. "Note on the Geometry of the Siderostat."

The following gentlemen were elected Fellows of the Society:—

J. S. Chakravarti, M.A., Assistant Accountant-General, Allahabad, India.

Chas. Sidney Mence, 49 Watling Street, E.C.

Rev. John Stutter, O.S.B., Acton Burnell, near Shrewsbury.

Ernest Geo. Wainwright, B.A., St. John's College, Battersea, S.W.

The following Candidates were proposed for election as Fellows of the Society:—

Spencer L. Fletcher, 38 Lammas Road, Ealing, W. (proposed by Rev. W. J. B. Roome).

Prof. M. B. Snyder, Director of the Observatory, Philadelphia, U.S.A. (proposed by Dr. Isaac Roberts).

The following foreign astronomers were proposed by the Council as Associates:—

Prof. W. W. Campbell, Director of the Lick Observatory, California, U.S.A.

Prof. J. Scheiner, Astrophysical Observatory, Potsdam, Germany.

M. Ch. Trépied, Director of the Observatory, Algiers.

THE BRITISH ASTRONOMICAL ASSOCIATION.

The sixth ordinary Meeting of the eleventh Session of the British Astronomical Association was held at Sion College on April 24, *Captain Wm. Noble* (Vice-President) in the Chair.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of five new Members was confirmed.

Several notes and papers on Nova Persei were read. *Capt. Markwick*, Director of the Variable Star Section, reported that he had received 299 light-determinations for the thirty-eight days from February 21 to March 31, giving an average of nearly eight per diem. The greatest number on one day was 23, on March 3. All the observations had been plotted, and a curve drawn, and this showed the principal fluctuations of light.

The writer then described the variation of light of the Nova, and stated that the large fluctuations after March 18 corresponded somewhat with the type of variation of S Sagittæ or T Monocerotis, for which an orbital cause was generally suggested, but he would not venture to say that this was the cause of the variation of Nova Persei. A large number of colour-determinations had been sent in, and the general inference to be drawn was that the Nova was undoubtedly white when at or near maximum. This did not last long, for on February 27 the first note of red was recorded, and after that it seemed to have rapidly developed into a highly-coloured red star with alternations of orange. Mr. Astbury, who gave special attention to colour, pointed out that during the great fluctuations, March 18–31, each increase of brightness was accompanied by a corresponding increase in redness

Mr. N. Maclachlan contributed a paper in which he detailed numerous observations of the Nova. The chief points to be noted were the dullness of the disc, the indistinctness of the image in the telescope, and the red colour of the Nova as intensified by the elongated focus. Of these, the first two seemed to indicate that the object was to some degree of a nebulous or similar nature.

Some notes on the spectrum of Nova Persei by the *Rev. W. Sidgreaves*, previovsly communicated to the North-western Branch, were also read.

Mr. Holmes read a note on "Temporary Stars," in which he said there was a general acceptance of the theory that temporary stars were the result of collisions in some form or other, but there was not, he thought, uniformity of opinion as to what were the particular bodies which collided. Without losing sight of the fact that at present we could not be sure that collisions were a full explanation, and without losing sight either of the fact that temporary stars exhibited variety in their manifestations, which implied variety in origin, Mr. Holmes proceeded to consider the various kinds of collision that had been suggested, and some facts bearing on them. He suggested that Monck's theory, that temporary stars resulted from the passage of a star through a nebula, had most in its favour. It would account for a rapid increase to a maximum which amounted to a sudden appearance; for its decadence, and, according to the length of its course through the nebula, for either short or long duration; while the variations of density which we knew to exist in various parts of many nebulæ would account for changes in brilliancy during its apparition, and it also accounted for the apparent transformation of temporary stars into nebulæ when approaching extinction.

Mr. W. T. Lynn said it was a question amongst astronomical photographers whether nebulous matter was of the great extent contended for by some; but however that might be, it was true, as Mr. Holmes said, that nebulous matter must occupy a far greater space in the whole heavens than stellar matter. He quite agreed with the view that stars were more likely to pass through some part of a nebula than to collide with other stars. In reply to the chairman, Mr. Lynn remarked that Dr. Roberts had said he did not think nebulous matter was so very widely diffused as was claimed by some photographers of late years.

Mr. C. T. Whitmell said that to estimate the velocity at which a star was approaching or receding, they must have a very definite observation of some spectral line, as otherwise it would be impossible to get a true estimate.

Mr. Crommelin said Mr. Holmes's paper had placed in a very vivid light the exceeding smallness of the combined disc of all the stars we could see. He (the speaker) certainly never realized before what an astonishingly small thing the disc would be—only about one-third of a second of arc, or a good deal smaller than the apparent size of Neptune, which required a good telescope to show a disc at all. Mr. Holmes had shown how much more probable it was that a collision should take place with nebulæ; but a little difficulty occurred to him, namely, that the nebulæ were so big that the passage of a star through a nebula would be a matter of many years, if not centuries. It would certainly take centuries for a star travelling at an ordinary pace to go through the nebula of Orion; and as these Novæ lasted only for a few months, and were at their brightest for only a few days, it would seem that whatever a Nova was colliding with, it could not be anything like the size of the big nebulæ. They thus lost an argument in favour of the collision on account of the great size of the thing collided with.

Mr. Holmes said it was not necessary to suppose that temporary stars always went through nebulæ of such extent as that of Orion.

Mr. Maw remarked that it was not necessary to suppose the star would traverse the whole nebula. There was no reason why there should not be a collision with the fringe of the nebula.

The *Rev. S. J. Johnson* contributed a paper entitled "Early Visibility of the New Moon," in which he mentioned that the new Moon on April 19 was just visible to the naked eye when barely 22 hours old. On February 8, 1864, he noticed the crescent 23½ hours old in the smoky climate of Lancashire.

Mr. C. T. Whitmell read a paper entitled "A Martian Sundial," describing from the conditions obtaining on Mars what would be the result of depending upon dial-time on the planet as compared with clock-time.

Mr. Lynn read a paper "On the Habitability of the Planets of the Solar System." He referred to the probability that Mercury, and possibly Venus, rotated in the same time as that in which they revolved round the Sun, in which case they could hardly be regarded as habitable. He also mentioned the late Mr. Proctor's theory that the most likely bodies in the Solar System besides our Earth to be abodes of life were the satellites of Jupiter, pointing out, however, that there was reason to believe that they rotated, like our Moon, in the same time that they revolved round their primary.

The *Rev. C. D. P. Davies* read a paper on "The Escape of Gases from Planetary Atmospheres."

Mr. Crommelin said it certainly seemed to him that some people had spoken a little too confidently about the escape of this or that gas from other planets. He did not think the question had really been solved with at all sufficient accuracy to do more than assert vague probabilities.

Mr. Whitmell remarked that Dr. Johnstone Stoney's hypothesis had been strongly criticised by Dr. Bryan, F.R.S., in papers read before the British Association. Some of Dr. Bryan's conclusions were completely at variance with some of Dr. Stoney's. The subject was a very difficult one—upon the very limits of knowledge—and it must be left for experts to settle.

THE ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday afternoon, May 15, at the Society's rooms, 70 Victoria Street, Westminster, *Mr. W. H. Dines*, President, in the Chair.

Mr. Rupert T. Smith read a paper on "The Periodicity of Cyclonic Winds," which was the result of a discussion of his own observations made in the neighbourhood of Birmingham during the 26 years 1874–1899. The equinoxes do not appear to be very stormy periods, but from the author's tables it is shown that the greatest frequency and force of cyclonic wind occurs some two weeks before the spring equinox and some three weeks after the autumn equinox.

Mr. W. Marriott gave an account of the bequest by the late Mr. G. J. Symons, F.R.S., to the Royal Meteorological Society. Mr. Symons (who died on March 10th last year) was the founder of the British Rainfall Organization, and was a most distinguished meteorologist. He took a great interest in the welfare of the Society, and held the office of Secretary for a period of 25 years, and was twice elected President.

By his will Mr. Symons bequeathed to the Society his cross of the Legion of Honour, the Gold Albert Medal awarded to him by the Society of Arts, the Testimonial Album presented to him in 1879 by the Fellows of the Royal Meteorological Society, and the sum of £200, as well as such of his books, pamphlets, maps, and photographs of which there was no copy in the Society's library. Mr. Marriott stated that from Mr. Symons's valuable collection he had selected for the Society over 5000 books and pamphlets, and about 900 photographs. A large number of the books were old and rare works, 750 bearing dates previous to 1800, while 8 were as early as the fifteenth century. By this noble bequest the Royal Meteorological Society now possesses the most complete and extensive meteorological library in existence.

The Solar Eclipse.

From the comparatively brief telegrams already received it appears that the success of the observations in Sumatra, and in less degree in Mauritius, has been somewhat qualified by cloud, but it seems that a considerable amount of spectroscopic work has been achieved and that any failure may only be expected in the long-exposure photographs. Mr. Dyson has kindly kept us informed of proceedings in Sumatra, and the following facts are extracted from his letters and from reports in the daily press.

The British representatives, Mr. Dyson, Mr. Newall, and Mr. Atkinson, arrived at Sumatra on April 6, and were received by Major Müller, the Chief of the Dutch Survey, who advised as one station the island of Aoer Gedang, about six miles from the west coast of the mainland of Sumatra, and 30 miles south of Padang. Mr. Joekes, the Governor of the West Coast, at once put himself and the Dutch Government launch at the disposal of the observers, and after seeing the island Mr. Dyson elected to pitch his camp there; so he and Mr. Atkinson, with the help of the crew of His Majesty's gunboat 'Pigmy,' under the command of Lieut. Oldham, set up their huts and telescopes on this small island about a mile in circumference, surrounded by a coral-reef and covered with palm-trees, with about a dozen natives as their only neighbours. The nearest observing-party was the Dutch, who were established on the main island at Karang Sago, close to Painan, on the coast. Mr. Newall went inland to Doerian, above the Oembilien coal-mines, near Savah Loento, at the extreme end of the railway. Here he found a friend in Mr. van Lessen, the engineer of the mines, who put a house at the disposal of Mr. and Mrs. Newall, with a convenient terrace on which to set up the instruments. The British parties were thus at the two ends of the line of observers in Sumatra, the others being disposed between them. There were on the island, besides the Englishmen, one Dutch party, four American, one Russian, one French *, and a Japanese expedition. Prof. Barnard, with expeditions from the Yerkes Observatory and the Smithsonian Institution, were at Solok; Dr. Humphreys at Fort de Kock, not far from the northern limit of the shadow; whilst the observers from Lick, Professors Shin Hirayama and Seiji Hirayama, of Tokio, and others were at Padang.

The telegram received immediately after the eclipse states that the eclipse was observed by the English astronomical party, assisted by the officers of the 'Pigmy' from Aoer Gedang, that the eclipse lasted six minutes twenty-one seconds, the Sun being partially obscured by clouds so that the operations were largely hampered, but Mercury, Venus, and Aldebaran were well seen, and that the corona resembled that observed in 1867.

The corona of 1867, it will be remembered, was of the sun-spot-minimum type, with long and straight equatorial streamers, and somewhat profuse at the solar poles.

The report as to weather from the Dutch party was to the same effect. Their photographs with the prismatic camera, and the measurements of polarization of light and heat-radiation of the corona, did not succeed.

A report from Mr. Newall, dated May 23, by which date he no doubt had had opportunity to acquire more detailed information, states that the weather was moderately good at all stations in Sumatra, it being best at Fort de Kock, and least favourable at Solok, which was unfortunate, as Prof. Barnard's programme required, possibly more than any other, a perfectly clear sky. It seems that his long-exposure photographs of the corona are quite spoiled, for which he will have the sympathy of everybody. Mr. Newall himself obtained good results with the grating spectroscope; his visual observations showed the green coronal ring very uneven. He got eight photographs with polariscope camera, which exhibit marked polarization of the brightest parts of the corona, especially at the southern edges of the equatorial streamers. His photographs of the corona are good, as are also those of Mr. Dyson; but Mr. Newall was again disappointed in the attempt to determine the rotation of the corona. The American observers at Sawah Loento measured totality as lasting $5^m\ 47^s$ instead of $5^m\ 42^s$ as predicted; but as the data on which this prediction is based are not stated, these figures are at present enigmatical.

In Mauritius the weather seems to have been more favourable, although it promised badly. The Sun rose in cloud, and the first contact was lost, but totality took place in a perfectly clear sky. In all, 52 photographs of the corona were taken, 18 spectrum-photographs, besides 41 of the partial phase.

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* See pp. 251 and 253.

Magnetic Observations during Total Solar Eclipse *.

THE effect produced by a solar eclipse on the meteorological conditions of the atmosphere has on many occasions in the past been the subject of observation, but in the number of *Terrestrial Magnetism* just received we find an account † of a systematic examination of the influence of such an eclipse on magnetic conditions also. It had appeared to Dr. Bauer, chief of the U.S. Magnetic Survey, that magnetic observations might on such an occasion be usefully undertaken; and the occurrence of the solar eclipse of May 28 of last year, the total phase of which was visible in the United States, afforded an excellent opportunity of carrying such design into execution. For the needs of the magnetic survey simultaneous magnetic observations are made on certain days throughout the year at the different magnetic stations, and it was arranged that such observations should be made, on the day of the eclipse, at stations as near as possible to the path of totality. Six stations were selected: three of them—Union Springs, Rocky Mount, and Cape Charles—were situated within the path of totality, the remaining three—Salem, Bayard, and Gaithersburg—being outside. The observers received instructions to occupy such stations as their special work permitted for the due accomplishment of the object in view, accompanied by a detailed scheme of the observations to be made. The prescribed course was carried out by all the observers excepting the one at Gaithersburg, who for some reason failed to receive his instructions in time; but he made observations according to directions sent him previously, relating to other work. The detailed scheme of observations is given with the view of aiding observers making preparations for similar work on future occasions. The observations made are discussed at considerable length, being accompanied by numerous graphical illustrations, and it is stated that there can be no question that some kind of magnetic disturbance made itself felt on May 28 at every one of the stations.

Finally, the conclusions arrived at are given under eleven separate heads, the principal points of which are contained in the following summary:—A small magnetic oscillation made itself felt at various stations situated in the eastern part of the United States during the time of the eclipse. It was detected by various persons, at various stations, with different instruments, under different conditions, and was also automatically recorded. The various phases of the oscillation did not take place at the different stations at the same absolute time, or local time, but in every instance were associated with the time of maximum obscuration of the Sun. The duration of the oscillation was apparently about the same as that of the eclipse, about two and a half hours. The range of the oscillation was about one minute in arc for declination and about eight units in the fifth decimal C.G.S. for horizontal intensity, that is, to about 1/2800th part of the absolute horizontal intensity. The general effect was to deflect the declination needle to the west, and decrease the horizontal intensity, before the time of maximum obscuration, the movement afterwards being in both cases in the opposite direction. The analysis indicates that the cause producing the magnetic oscillation was situated outside of the Earth's crust, the presumption being very strong that the oscillation is to be referred to some change produced in the upper atmospheric regions by the abstraction of the Sun's rays, due to interposition of the Moon.

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* From *Nature*, 1901 May 2.

† *Résumé* of magnetic observations made chiefly by the United States Coast and Geodetic Survey on the day of the total solar eclipse, May 28, 1900.

Dr. Bauer expresses himself as having been in doubt before making the observations as to whether any magnetic effect referable to the eclipse would reveal itself, and adds that he was afterwards slow to conclude that the magnetic oscillation observed was not accidentally connected with the eclipse, until he had made such exhaustive examination of every point involved as justified him in formulating a definite conclusion. The result is interesting, and makes it desirable, as he says, that every opportunity should in future be taken to obtain, during eclipses, simultaneous magnetic, atmospheric-electric, and meteorological observations at as many stations as possible.

It is to be remarked that, although Dr. Bauer eventually speaks with some confidence as to the magnetic movement observed having relation with the eclipse, the movement in question was small, and, abstractedly speaking, much too small on which to found any certain conclusion, considering the abundance of magnetic movements of similar and even greater magnitude. The circumstance that seems really to give weight to the conclusion drawn is the statement that the various phases of the magnetic oscillation were associated with the time of maximum obscuration of the Sun. Confirmation of this circumstance is therefore what is now to be desired.

Following the paper there is printed an appeal for international co-operation in magnetic and allied observations during the total solar eclipse of May 17 next. WILLIAM ELLIS.

Variability in Light of Eros[*].

THE discovery by Dr. Oppolzer that the light of Eros is variable suggests some photometric problems of great interest. If, as seems probable, we assume that the variation is due to the rotation of the planet, we can, from measures of its light determine the time of rotation, and the direction in space of the axis of rotation.

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[*] Extracted from 'Harvard College Circular' No. 58.

Owing to the varying position of the observer with regard to the planet, much information can be obtained which is impossible in the case of a variable star.

Four corrections must be applied to the observations. First, for the velocity of light; second, for the distance of the Sun and Earth; third, for phase; and fourth, for the direction of the axis of rotation. If this axis were pointed towards the observer, no variation would be perceptible, while the range in brightness would attain its maximum value when the axis was at right angles to the line of sight. Neither of these conditions can be fulfilled exactly, since the position of the axis is probably nearly fixed, and the inclination of the orbits of Eros and the Earth would make great changes in this angle. Let p represent the complement of the altitude of the Earth above the equator of Eros, which will be equal to the angle between the axis of Eros and the direction of Eros as seen from the Earth. Let v denote the angle between the plane passing through the Earth and the axis of Eros, and any other plane passing through the axis of Eros, assumed as an origin. A most important correction depends upon v. The time of all the observations must be corrected by an amount equal to v divided by 360° and multiplied by the period of variation. As a first approximation, we may assume that the axis of Eros is parallel to that of the Earth, and that the plane passing through the vernal equinox is taken as an origin. In that case, v will equal the right ascension of Eros. As stated above, if $p = 0°$, there will be no variation in light, and the range will be zero. If $p = 90°$, the range will attain its maximum value. For intermediate values of p, we may assume that the range will be proportional to cos p. The changes in the range may be used to determine the value of p, and from it the position of the axis of Eros. Equations may be formed in which p and v, or p alone, are the unknown quantities from which we may derive the approximate position of the axis. Besides observations at the present time, it will be necessary to determine the light-curve when Eros is in several other portions of the sky, determining the range and also the times of maximum and minimum as accurately as possible. The rapid motion of Eros renders it difficult to compare the observations on different nights, without using different, and in some cases distant, comparison stars. Fortunately, the change in light is so rapid that consecutive observations of a large part of the light-curve can generally be made. The opposition of 1894 would have been particularly favourable for these studies, since the declination changed from $+57°$ to $-14°$ in a few months, and would thus have furnished large coefficients for determining the value of p, although, as shown below, the range seems to have been small at that time.

Assuming that the variation in light of Eros is due to its rotation, two explanations may be offered, as in the case of variable stars of short period (Proc. Amer. Acad. 1881, xvi. 257). First, that Eros is darker on one side than on the other, as is probably the case with Iapetus, the outer satellite of Saturn; and secondly, that it is elongated, or double, as has been assumed by M. André and others (Astron. Nach. No. 3698). In the first case, the successive maxima would always have the same intensity, and would succeed each other at equal intervals which would be equal to the period of revolution. The same would be true for the minima. In the second case, if the two bodies differed in diameter, the successive maxima and minima might have unequal intensities, and if the orbit were elliptical the intervals between them would be alternately long and short. This seems to be the case with Eros, and the first hypothesis seems therefore improbable.

On the other hand, if the variation in light is caused by two similar bodies alternately eclipsing each other, it is difficult to see how more than half the light can be cut off in each case, and the minima more than three-quarters of a magnitude fainter than the maxima. It then becomes necessary to assume that the two bodies are of unequal brightness, that they are elongated, or that we have a single body of the shape of a dumb-bell. Some observers have found the minima two magnitudes fainter than the maxima. To account for this, we should be obliged to assume that one axis of the body was six times as long as that at right angles to it. Observations show that the light of Eros is continually varying, while if the case were that of a simple eclipse, as in the stars of the Algol type, we should expect that it would retain its full brightness for a large portion of the time.

If the bodies were of the same size, and the orbit circular, it might be impossible, from the light-curve, to distinguish between the two hypotheses. The fourth of the corrections mentioned above, however, furnishes a means of distinguishing between them in any case. If the body is dark on one side, the time of revolution will equal the interval between the successive maxima, and the correction for the position of the observer will be proportional to this quantity. If then the position changes 180°, the correction will be one-half the interval between the successive maxima. In the second case, the time of revolution will be double this, that is, equal to the interval between a given maximum and the next but one, so that the correction for position will now be twice as great as before, and approximately equal to the interval between the successive maxima.

Much material already exists for determining the constants mentioned above. Several of the photographs of Eros taken in 1893, 1894, and 1896 had an exposure of an hour or more. Owing to the motion of Eros, it formed a trail on each of these plates, which in some cases shows distinct variations in brightness. This was noticed when the plates were first examined, but was supposed to be due to changes in the haziness of the air. As this is an easy method of discovering the variability of an asteroid, it is hoped that astronomers engaged in a photographic search for such objects will examine carefully all trails, to detect any changes in intensity. An examination of forty-one asteroid trails photographed with the Bruce telescope, seven of them on a single plate, failed to show, except in one or two instances, any change beyond that apparently due to varying atmospheric absorption. Generally, more than one asteroid appeared on each plate, and in such cases all showed the same changes in intensity.

E. C. Pickering.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Eclipse of Agathocles.

Gentlemen,—

So much has been written about this eclipse that it is with great diffidence that I add a few words more. I was induced so to do by reading the late Prof. Freeman's reference to it in the fourth volume of the *History of Sicily*, which was left incomplete in consequence of the lamented author's death in 1892, so that the latter portion was brought out from posthumous manuscripts under the editorship of Mr. A. J. Evans, M.A., Keeper of the Ashmolean Museum. Chapter xii., containing the account of Agathocles, commences on p. 356 of vol. iv., and his first expedition to Africa on p. 400. Agathocles was enabled to avoid the blockading fleet of the Carthaginians by the approach of a convoy of provision-ships, which they went to intercept. When they found that Agathocles had sailed from the harbour, they abandoned that object in order to attack him, so that the ships probably arrived safely, as the American provision-ships did at Brest in 1794, escaping Lord Howe's fleet in consequence of its attack by that of the French, leading to the famous action of the 1st of June. Agathocles, however, escaped under the darkness of the night and made the African coast after a six-days' voyage. On the next morning after he left Syracuse, we are told, both by Diodorus Siculus and Justin, that an eclipse of the Sun overshadowed them in such a way that the stars appeared as if it had been night. There appears no reason to doubt that this was the total eclipse of August 15, B.C. 310. But there has been some discussion as to the exact place in which the eclipse found Agathocles and his fleet. It is naturally generally supposed that he rounded at some distance the south-eastern corner of Sicily and approached the Carthaginian territory in that direction. Airy, however, in his paper on the subject in the *Philosophical Transactions* for 1853 (vol. cxliii. part 2, p. 187), adopts a view which, he says, was suggested to him by J. W. Bosanquet, that Agathocles passed through the Straits of Messina and along the northern coast of Sicily, finally approaching

Africa from the north-west. For the adoption of this longer route he gives two reasons. First, even that was longer than a six-days' voyage would be likely to have occupied. Secondly, the provision-ships in all probability came from Gela, and therefore approached Syracuse from the south; the Carthaginian fleet therefore to intercept them would move in that direction, and Agathocles to avoid it would go northerly. That, however, would look like flight, and the first idea of the Carthaginians, when they saw that he had left the harbour, seems to have been that Agathocles intended to offer battle. To me it seems most likely that he simply stood out to sea towards the east (which happened to be in the track of the eclipse), and when he found in the morning that he was out of reach of the enemy's fleet, he then sailed the nearest way to Africa. That he landed near Cape Bon, and not, as Baily supposed, much further to the south in the Gulf of Khabes, I believe all authorities are now agreed. As regards the length of time occupied in the voyage, it is worth remembering that Nearchus (whom Agathocles afterwards sent from Tunis to Syracuse to convey the news of his victory) took, having every object to use the utmost speed, nearly as long. Before Agathocles landed he was attacked by a Carthaginian fleet, which both Grote and Freeman assume was the blockading force which had pursued him; it may, however, have been a different fleet, as Airy suggests. Had it been the same, they must soon have conjectured his true destination; had it been different, it was probably smaller and more easily beaten off.

The question of the route is of interest astronomically, as more nearly fixing the path of the Moon's shadow. Prof. Ginzel makes it in his map pass over nearly the whole of Sicily and then in a direction slightly north of east to Greece, the central line passing near Syracuse and then to northern Greece and over the Ægean Sea to Asia Minor. In any case, Agathocles must for some minutes have been immersed in the shadow.

It is perhaps a small matter, but astronomers, like other people, may as well get what amusement they can out of their work, and it does excite a smile to find the extraordinary remarks sometimes made by even great writers when out of their own line. Prof. Freeman once wrote me, in answer to a communication respecting a comet, that he knew nothing of astronomy; and in a note on our present subject at p. 401 he writes: "Dr. Julius Zech fixes the date as 'Aug. 14, —309.' By 309, however, he appears to mean B.C. 310 (according to the antiquated French way of reckoning), and thus differs from other astronomers by a day only." That —309 = B.C. 310, and that astronomers of all nationalities *do* and *must* take it so, does not seem to have occurred to Freeman; also August 14 is astronomical reckoning for the morning of August 15 civil reckoning.

Yours faithfully,

Blackheath, 1901, April 30. W. T. Lynn.

Velocity of α Persei in the Line of Sight.

GENTLEMEN,—

In the *Observatory* for May 1901 (No. 305), Mr. W. W. Campbell publishes the observations of *α* Persei obtained with the Mills spectrograph, which do not afford any evidence of variable velocity of the star in the line of sight. The observations of *α* Persei, made at Potsdam soon after Mr. Newall's announcement of the variable velocity of the star, had the same results, which I published in *Sitzungsberichte der königl. Preussischen Akademie der Wissenschaften zu Berlin*, Januar 1901.

Since a good many of the separate copies of my paper which I sent by post to foreign astronomers seem to have been lost, I should be very much obliged to you if you would reprint my results.

Observations of *α* Persei.

Date, 1900–1901.	Potsdam M.T.		Velocity * rel. to ♂.	Red. to ⊙.	Velocity * red. to ⊙.
	h	m	km.	km.	km.
Dec. 13	7	56	+ 7·8	− 9·4	−1·6
14	7	11	+ 6·2	− 9·8	−3·6
18	5	7	+ 8·8	−11·4	−2·6
20	5	5	+10·6	−12·2	−1·6
21	9	17	+ 9·3	−12·7	−3·4
22	5	21	+10·2	−13·0	−2·8
Jan. 1	5	5	+12·4	−16·8	−4·4
2	5	1	+13·4	−17·2	−3·8
3	5	10	+12·6	−17·5	−4·9
4	5	1	+14·4	−17·8	−3·4
5	4	55	+13·8	−18·2	−4·4
9	5	18	+16·6	−19·4	−2·8
9	6	32	+16·9	−19·5	−2·6
					−3·22

These results may be considered as preliminary ones in regard to the absolute value of the velocity, because I have measured the plates under the microscope only in one direction.

Potsdam Roy. Observatory,
1901, May 14.

H. C. VOGEL.

Nova Persei.

GENTLEMEN,—

Four of my observations of the magnitude of the new star given below are available for filling up gaps in the list of magnitudes on p. 207, viz., those of April 3, 6, 13, and 14. That of April 3 supplies an apparently missing minimum. It was obtained (with the aid of a binocular) with some difficulty, on account of the full Moon, but I think it cannot be much in error. The other observations are given for comparison. All agree very well with the magnitudes given on p. 207, except that of March 25; but perhaps when the time is taken into consideration this discrepancy may disappear to some extent.

Further observation has been impossible here, as the Nova now goes below the horizon before dark.

The comparison-stars used were ε, δ, ν, 32 (*l*), 30, and 36 Persei; H.P. magnitudes.

Date.	G.M.T.	Mag.	Date.	G.M.T.	Mag.
March 7	8.30	3·0	March 24	9.00	4·2
8	10.00	3·1	25	10.35	4·8
12	9.00	3·3	April 3	10.00	5·4
13	8.40	3·8	4	9.00	4·4
14	8.30	3·8	6	8.50	5·4
16	8·40	3·9	13	9.00	4·4
20	8.40	3·8	14	9.15	5·4

The Nova was very near the (mountain) horizon when the last observation given was made, but I believe it to be reliable.

Yours sincerely,

Madeira, 1901, May 13. Wm. Anderson.

The Great Inequality of Jupiter and Saturn.

Gentlemen,—

The discovery of the great inequality between Jupiter and Saturn is sometimes attributed to Halley, who has a remark in the 'Astronomical Tables,' published some years after his death, on the lengthening and accelerating of the motions of Jupiter and Saturn about the end of the seventeenth century, which he suggests is due to the fact that those planets were in conjunction in the year 1683. "If," he says, "the same happen again and again, when *Jupiter* and *Saturn* are in conjunction in Leo, we may justly hope that the errors we find in their motions, as they are owing only to the joint efficacy of these Centers, at length may be removed by the *Newtonian Geometry*. But if not, or if the Periods should prove longer where they are now the shortest, or the contrary, there must be some extrinsic cause, of which we are now ignorant."

It was left for Laplace to explain how the near commensurability of the periods of these planets produced, in accordance with the Newtonian law, that periodical oscillation which is known by the name of the great inequality. A similar one, of smaller amount, between the motions of the Earth and Venus, called their long inequality, was long afterwards pointed out by Airy.

Newton himself, however, appears to have been the first to call attention to the mutual effects of the attractions of Jupiter and Saturn, when these were increased by their coming into conjunction. According to M. Sigorgne, this convinced even Flamsteed at last of the truth of the Newtonian theory. In the preface to his 'Institutions Newtoniennes,' second edition (1769), p. xxxix, after speaking of the comparatively small effects of the perturbations produced by Mars, the Earth, Venus, and Mercury, he says:—

"Il n'en est pas de même de Saturne et de Jupiter. La masse de celui-ci est si grande, que le calcul démontre qu'elle doit opérer des effets sensibles sur le mouvement de Saturne dans le tems de leur conjonction. *Newton* en avertit les Astronomes *Flamsteed* et *Hallei.* Mais le premier n'eut aucune foi à la prédiction: cependant le tems de la conjonction de ces deux planètes arriva; on fit pour la première fois cette observation singulière, et le calcul fut vérifié. Cela valut à la Philosophie Newtonienne le suffrage d'un aussi grand Astronome que *Flamsteed.* Eût-il été possible qu'il ne se fût point rendu?"

It certainly would seem from passages in Flamsteed's letters to Sharp, dated March 2, 1707-8, and September 20, 1710 (Baily's 'Flamsteed,' pp. 266, 277), that he did at last perceive something of the way in which gravitation acts, though we must probably conclude with Whewell ('Newton and Flamsteed,' p. 7) that he "never fully accepted Newton's theory, nor comprehended its nature," but ". . . . understood by *theory* only a mode of expressing *laws of phenomena,* not a new generalisation by which such laws are referred to a physical *cause.*" Hence it might have been that he does not seem to have perceived that the exactness of the theory was not affected by the fact that, in working out lunar and planetary tables therefrom, it was necessary accurately to determine the masses as well as distances of the attracting bodies, which could only be accomplished by long-continued observations.

Yours faithfully,

Blackheath, 1901, May 22. W. T. LYNN.

P.S.—It may be worth while to correct here an error of Delambre, who in his 'Histoire de l'Astronomie au dix-huitième siècle,' p. 100, referring to the 'Historia Cœlestis Britannica,' writes:—

"L'édition est dédiée au roi George, par Marguerite Flamsteed, veuve, et Jacques Hodgson.

"Je croyois que cette veuve était celle de Flamsteed; mais le Dictionnaire historique dit que Flamsteed mourut dans le célibat. Alors Marguerite serait probablement la sœur ou une autre parente de Flamsteed."

His widow, however, notwithstanding the 'Dictionnaire,' she certainly was.—W. T. L.

Republication of Astronomical Papers.

GENTLEMEN,—

I was glad to see the remarks in your last number on the republication of astronomical papers in the *Astronomical Journal.* Such republication is often of the greatest use to the readers of the journal. But I hope it will not be confined to recent papers. For example, I have never seen the paper in which Mr. H. Lane is said to have proved that if the Sun were gaseous throughout, its temperature would rise while it was contracting in consequence of the loss of heat. This result is so startling that I think many persons beside myself would wish to see the reasoning by which it is arrived at. As far as I know, the contraction of the gas only sets in when the temperature is lowered, and ceases when the temperature ceases to fall. (I do not deny, however, that the heat generated by the falling-in of the particles towards the centre of gravity may greatly retard the cooling process.)

This is one example out of many. Most of your readers have read in text-books a statement that somebody has proved something, often without any reference, and when a reference is given it is perhaps to some publication which is practically unprocurable. Would it not be desirable to republish a number of papers in which important results are supposed to have been established, but which originally appeared in periodicals not easily procurable, and of which the books in ordinary use only give the results? I confess that in some cases which I have examined I think there has been too great readiness to accept such results as beyond question, and that it is of great importance that analyses and arguments which have hitherto been submitted to only a small circle of astronomical readers should be submitted to criticism in a work which is more generally read. Truly yours,

Dublin, 1901, May 13. W. H. S. MONCK.

OBSERVATORIES.

UNIVERSITY OBSERVATORY, OXFORD. — It has been already explained in this magazine that the Savilian Professor does not yet enjoy the advantages of a house attached to his observatory, but his report for the year ending April 30, 1901, shows that he has been able to effect certain structural and decorative alterations. The electric light is being put into the observatory, and a new dome, 23½ feet in diameter, was erected by Messrs. Cooke and Sons in the months May to August 1900, over the Astrographic Equatorial, the following facts about which are of interest:—

The shutter opening is 7¼ feet at the horizon and 3¾ feet at the zenith. The dome can be completely rotated in two minutes, but this involves some exertion, owing chiefly to the inconvenient position in which the turning gear is placed, 7¼ feet above the floor. The fact, however, that any portion of the sky can be reached from any other in less than one minute is sufficient. The dome weighs four tons, and is consequently steady in high wind. During the gale last December not the slightest motion was noticed. On the other hand, the wide shutter opening renders the telescope liable to disturbance from wind, which was never noticed with the old dome. As regards vibration from other causes, jumping on the floor causes no perceptible tremor in a star-image. But if the dome be touched (as, for instance, if the cog-wheel of the turning gear be turned through the "slack" between one tooth and the next, without actually moving the dome) the star-image dances wildly. This means that the vibration travels down the wall carrying the dome to the foundations, through the ground to the foundations of the telescope pier, and up through this pier (23 feet high) to the telescope. The pier is, of course, isolated from the floor, and jumping on the floor seems to shake neither telescope nor walls. As a curiosity it may be remarked that, when the opening of the dome is pointed to Norham Gardens, the other side focusses a most elaborate echo, which will repeat correctly twenty or thirty syllables.

Owing to interruption of the photographic work by the erection of this dome, and the necessity for observing Eros and the new star in Perseus, the record of the work on the 'Astrographic Catalogue' is not quite so full as usual, but a considerable amount has been done. According to Prof. Turner's convenient schedule, the total for last year was 10 per cent. of the whole work on the Catalogue, which brings the amount now done to 72 per cent. of the whole. The printing of the Catalogue is under the consideration of the Delegates of the University Press. No chart-plates have, we believe, yet been taken at Oxford, and the Savilian Professor hints that if these and their reproduction are to be undertaken for the Oxford Zone the work should be considered a national one, and the Government should find the money. The University Observatory has taken part in the campaign for finding the parallax of Eros, and contributes 114 plates with 757 separate exposures. 300 images of Eros with the stars around it have been measured involving 22,000 separate measures.

Prof. Turner explains why he did not go to Sumatra to observe the eclipse—the essential reason being that there are statutory difficulties in obtaining leave of absence for an aggregate period of more than eight weeks in two consecutive years, and the eclipse journey with that of last year would have more than covered that period.

PUBLICATIONS.

GREENWICH SECOND TEN-YEAR CATALOGUE OF 6892 STARS FOR 1890.—In the contemplation of a new Star-Catalogue the fundamental considerations that suggest themselves are of three distinct classes, and refer to subject-matter, quality, and quantity, though there may be some diversity of opinion as to the relative importance of these considerations.

As to subject-matter, which in many, if not all, previous

Greenwich Catalogues has been practically ignored in writing the Introduction, so that the student has been rather at a loss to find reasons for the presence or absence of particular stars, we have in the present work a statement of scope, which, though it points out an omission mentioned below, is certainly an advance.

Leaving out fundamental stars, a large number of which are an essential in any fundamental catalogue, we infer that the original scheme of the work was the re-observation of Groombridge's stars, which had once been done at the Radcliffe Observatory for the epoch 1845 (Groombridge's epoch being 1810), supplemented by a large number of fairly bright stars in other parts of the sky, Groombridge's being mostly circumpolar.

The following passage— "Of the 4243 stars in Groombridge's Catalogue, 3645 are contained in this catalogue, and the remainder, with few exceptions, have been observed in the preceding Ten-Year or Nine-Year Catalogues"—requires explanation. It may fairly be asked why it should have been thought not worth while to include the whole number in the present catalogue, and why in any case the "few exceptions," supposing the stars to have a real existence, were permitted.

If one may hazard a conjecture, it would be that since the total number of stars in the previous Ten-Year Catalogue was only 4059, which represented a great advance on its predecessors, it was thought to be too high an aim to include the whole of Groombridge's stars, and that the exceptions referred to were accidental.

Besides these Groombridge stars, it appears that several hundred stars were included whose positions were required as guiding-stars for the Astrographic Equatorial or as comparison-stars for comets, or which had been occulted by the Moon, also zodiacal stars, southern stars observed for refraction, and 340 Piazzi stars.

As to the quality of the observations, a casual glance at the diagrams which form the frontispiece will show how effective has been the device of 12-hour groups, first introduced in the Nine-Year Catalogue, for correcting the Standard Right Ascensions of the clock-stars. It is not clear, however, that this is entirely due to an improvement in the observations, or in the standard R.A.'s, as very possibly something is due to the increased numbers and proportions of stars in 12-hour groups, the figures being for 1880, 3223 out of 7904, and for 1890, 6128 out of 12599. If the other observations of Right Ascension are as good as those of the clock-stars, there would seem to be very little fault to find with them, though they suffer, according to the Introduction, from the very small effect of an erroneous collimation, from a diurnal variation of level error (which, though small, would very possibly account for the R—D. discordance in R.A. which is noted), and from possible diurnal variation in azimuth also, since the azimuth error is said to depend essentially on double transits of Polaris. The equinox of this catalogue is that of the Greenwich 1872 catalogue, carried on by the use of Struve-Peters precessions and

Auwers-Bradley proper motions. A correction to this zero has been found each year from the observations of the Sun, which show in the mean a correction $+0^{s}\cdot047$ to be applied to the Right Ascensions of this catalogue; but as personality may have a large share in causing this quantity, it has not been applied.

In polar distance the observations suffer slightly from the diurnal variation of the nadir-point, from the much-discussed R—D discordance (which is here treated at length, but which may well be made up of too many small quantities to be allowed for by a simple rule), and from the uncertainty attaching to the determination of co-latitude and to the refraction-tables. These drawbacks are, however, very small, and are well known to exist in every transit-circle catalogue up to the present time—though there is hope for the future, especially in the direction of the determination of azimuth-error, co-latitude, and refraction.

An exhaustive discussion is given of the discordances of observations above and below pole, and of discordances between the present Catalogue, Newcomb's Fundamental Catalogue, the Cape Catalogue, and Radcliffe Catalogue of the same epoch, and the various Astronomische Gesellschaft Catalogues for 1875, and some others.

As regards quantity, the number of stars (6892) speaks for itself, especially after a short analysis of some of the figures.

The great increase in the number of observations of the clock-stars has been referred to above, and it will be noted that there are only about 20 places depending on single observations in the present catalogue, as against about 150 in the First Ten-Year Catalogue, which contained in addition nearly 30 stars observed in only one element.

Other steps in advance are not wanting. A beginning has been made with the determination of proper motion for 174 Groombridge stars, and some definite attention has been paid to a few well-known binary stars, besides which known double stars to the number of about 250 are noted as such in the catalogue itself.

THE TOTAL SOLAR ECLIPSE, 1900 *.—The B. A. A. were unable on this occasion to arrange for a special steamer to convey the observers to their various destinations. Such an arrangement would have made it easier to carry out systematic series of observations, as was done in 1896, when the field of operations was carefully mapped out during the voyage, and large numbers of volunteers were found to assist in the various researches. But in 1900 the parties were very scattered, and there was little opportunity of co-operation, so that each party carried on its operations

* Report of the Expeditions organized by the British Astronomical Association. Edited by E. Walter Maunder. *Knowledge* Office. Price 7*s.* 6*d.*

independently of the others. The stations occupied by the Association were (1) Wadesboro', U.S.A., (2) R.M.S. 'Austral' off Portugal, (3) Ovar, (4) Plasencia, (5) Navalmoral, (6) Manzanares, (7) Elche, (8) Algiers, (9) Cape Matifou. This volume aims at being not a mere dry summary of results, but a popular and readable narrative. Hence about one-third of the book is taken up with incidents of the journeys of various parties, and the places of interest visited. Many excellent photographs of these are reproduced, the book thus serving to the observers themselves as an admirable memento of the expedition. We may quote an amusing little anecdote which Mr. Bacon tells against himself. They were crossing Virginia by night in a Pullman car: "I saw a burst of meteors twisting in erratic courses, much as I had seen them on a memorable occasion from a lofty balloon. . . . I shouted this intelligence aloud . . . . meeting, however, with a rebuke from our dusky and amused conductor, 'Dem are lightning bugs (fireflies), sah.'"

All the parties speak of the great courtesy with which they were everywhere received, in spite of the friction which the war was supposed to produce. The Portuguese Government may perhaps be singled out for special mention; "They franked all our instruments, lending tents, railing off spaces for us, and telling off guards. . . . They gave us first-class free passes over all the lines in Portugal."

The account of the eclipse proper begins with the times of contact. A large number of observations (not all by members of the B. A. A.) are collected by Mr. Whitmell. The Moon was evidently not more than 3" or thereabouts from the tabular place, but the N. A. duration of totality was certainly too long, as has already been pointed out.

The next section is "Drawings of the Corona" by H. Keatley Moore. There is no doubt that corona-drawing has improved in accuracy of late years. It may be that the draughtsmen are more careful, knowing that the photographs will detect their mistakes, or there may be greater familiarity with the various coronal types. The eleven drawings that are reproduced, though at first sight very dissimilar, are seen on examination to agree in essentials, though, as is almost inevitable in such hurried work, mistakes occur in the proportions of the various parts. They are combined in a beautiful drawing by Mr. Moore, which is in very satisfactory agreement with Mr. Wesley's combination-picture from the photographs. The eye has on this occasion beaten the camera, the western streamer having been seen distinctly beyond Mercury by more than one observer, while none of the photographs seem to show it as far as Mercury.

Mr. Wesley concludes, from a comparison of the American photographs with the others, that no perceptible change took place in the corona in 2½ hours. He discusses the curious dark rays, which are plainly shown on some of the photographs, and which have been thought to indicate some dark absorbing matter. Somewhat similar structures appear on the photographs of 1871, 1896, 1898.

Mr. and Mrs. Maunder note that the rod-like outer rays could not be traced to more than half the distance shown in 1898. This was probably due both to real faintness in the corona and greater sky illumination.

The book also deals with shadow-bands, Baily's beads, stars observed during totality, meteorological observations, colours on land and sky, &c. On all of these heads interesting and useful observations were made.

The book, while not recording any very sensational discovery, may claim to be a record of a large amount of careful and painstaking work, and no one who is interested in solar eclipses should fail to procure it.

A MANUAL OF ELEMENTARY SCIENCE *.—A member of the British Astronomical Association is reported to have read a paper at a recent meeting criticizing the average text-book on astronomy, which gives its readers dry facts of little use to them when confronted with the actual facts of Nature. It occurred to us that this gentleman was creating his own bogey in order to have the pleasure of demolishing it, or else his choice of books had been injudicious. We would recommend to his notice the book now before us, by Prof. Gregory and Mr. Simmons, which he may find more to his taste. The work is divided into three parts, devoted respectively to Physics, Chemistry, and Astronomy. The first two of these are scarcely in our province, but it occurs to us that in the earlier chapters there is much which is too elementary, although, as the book is evidently intended to help persons to pass certain examinations, the authors are, no doubt, well advised in making it so. To the section on Astronomy we can give unqualified praise. It opens with a chapter by the aid of which anyone can easily learn the names of the stars and the constellation figures in the sky, so that he may emulate Mr. Anderson in his recent discovery, and which will give him much useful information about the scheme of star-magnitudes which it would be difficult to pick up elsewhere. The other chapters give excellent explanations of the apparent motions of the heavenly bodies with very intelligible diagrams, of which we would call especial attention to that on p. 369, explaining why a different set of stars is seen in the sky at different times of the year. We can safely recommend this book to anyone who wishes to get a sound practical knowledge of science in its elementary stages.

---

* By R. A. Gregory, F.R.A.S., Professor of Astronomy at Queen's College, London, and A. T. Simmons, B.Sc. London: Macmillan, 1901.

NOTES.

Comet Notes.—The bright comet (*a* 1901) discovered by Mr. Halls on April 23 was independently discovered by Mr. Tattersall, of Cape Leeuwin, West Australia, and by Mr. Nicholson, of South Madagascar. Much uncertainty prevailed for some time about its movements, in consequence of various conflicting reports. The reported observation at Yerkes, referred to in our columns last month, proved to have been erroneous. The object they observed may have been *a* comet, but it cannot have been *this* one. For this comet was 13° south of the Sun at the time when they reported it 15° to the north of it.

Mr. Chambers reported that he had seen the comet's tail due east at 3 A.M. on May 1. But this observation also is incompatible with the subsequently ascertained place of the comet. In fact it was an *evening*, not a *morning* star at that date.

Several reported observations in England given in the daily press must also be set down as erroneous. No genuine observation of the comet seems to have been made further north than the Lick Observatory, where it was observed on May 15. In these latitudes the comet has only been above the horizon in daylight or bright twilight, and as its brightness has enormously diminished, and is still doing so, there seems no possibility of its being observed here.

The comet was observed in the evening early in May at the Cape and Arequipa, and these observations enabled an approximate orbit to be deduced. The following elements are by Prof. Kreutz, and are based on Cape observations of April 24, May 3 and 4:—

$$\begin{array}{ll} T \ldots\ldots\ldots & 1901 \text{ Apr. } 24{\cdot}26 \text{ Berlin M.T.} \\ \omega \ldots\ldots\ldots & \left.\begin{array}{l} 202^\circ\ 50' \\ 109\ \ 57 \\ 131\ \ 26 \end{array}\right\} 1901. \\ ☊ \ldots\ldots\ldots & \\ i \ldots\ldots\ldots & \\ \log q \ldots\ldots & 9{\cdot}3885 \end{array}$$

Ephemeris for Berlin Midnight (from later but similar elements).
'Centralstelle Circular' No. 44.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 3....	6	49	15	8	16	June 13....	7	10	58	9	29
5....	6	54	5	8	34	15....	7	14	47	9	41
7....	6	58	38	8	49	17....	7	18	30	9	51
9....	7	2	55	9	4	19....	7	22	6	10	1
11....	7	7	1	9	17						

This ephemeris shows that even in the southern hemisphere the comet will again be lost in the sunshine soon after the middle of June. Its brightness, taking that at discovery as unity, is

0·03 on May 24,
0·008 on June 11,
0·005 on June 19.

The comet may possibly be picked up with large instruments in August or September, but it will be very faint.

In the early days of its appearance it was evidently such a striking object as to arrest the attention of the most casual observer.

The *Journal of the B. A. A.*, No. 7, contains a letter from W. G. Levander, who is with the forces near Mafeking, stating that the comet was a beautiful object just above the rising Sun on the mornings of April 24 and 25.

But when it became an evening object observations were naturally more numerous. A telegram from Perim stated that the comet had been observed on board ship in the Red Sea.

A rough sketch taken in St. Helena on May 5, which we have seen, shows two narrow straight tails inclined some 10° to one another and some 10° in length, with a darker region between. On the other hand, Mr. Maunder and Mr. Claxton photographed it in Mauritius at about the same time, and stated that there were three tails, the longest some 15° in length.

The nucleus seems to have been small and stellar, thus admitting of accurate observation.

The *Times* of May 18 contains the following message, reported to have been sent from Louis Botha to his followers:—

> He owned he had thought of making peace with Lord Kitchener, but thanked God that he had rejected his terms. In 1881 a blood-red comet appeared, meaning war; the comet now seen was white and signified peace, which would shortly be given them, and with it independence.

The erroneous statement which appeared in the daily papers that the motion early in May was north-west, seems to us to have arisen from the fact that the tail was pointing south-east. It is natural for a novice to infer from the appearance that the comet is moving head first, and "carries its tail behind it," whereas, of course, in receding from the Sun the tail goes first.

Ast. Journ. 496–97 contains definitive elements of Comet 1894 II. (Gale), by Henry A. Peck.

The number of observations exceeds 500, the limiting dates being 1894 April 2 and August 21. We must take most serious exception to one point in the computation:—"Whenever a correction of the time of observation would harmonize otherwise discordant data, this has been done without calling attention to the fact." It seems to us only legitimate to alter a time after consulting the original observer, who is pretty certain that the alteration is right. Even in this case the alteration should be fully noted. But to alter a time arbitrarily, without any note, merely to make different observations fit better, is most flagrant "cooking"; in our complete ignorance of how many times this objectionable process has been carried out, we necessarily view the entire computation with some suspicion. It is far better to reject discordant observations altogether than to make arbitrary corrections to them.

Perturbations by Jupiter, Saturn, and the Earth have been applied.

We give his orbit below, the elements being affected by an uncertain variable $d\nu$, whose most probable value he considers to be $-40''$. We give Dr. Roseby's elements for comparison; these were deduced from a much smaller number of observations, ranging over only 2½ months (*Monthly Notices*, March 1896):—

	Peck.	Roseby.	
T	1894 Apr. 13·4069 $+0·0004\,d\nu$	Apr. 13·3933 G.M.T.	
ω	324° 12′ 22″·5 $+1·20\,d\nu$	324° 11′ 40″·5	1894·0.
☊	206 23 53 ·0 $-0·53\,d\nu$	206 24 11 ·9	
i............	86 59 18 ·2 $+0·85\,d\nu$	86 58 55 ·7	
q............	0·983093 $+·0000013\,d\nu$	0·983034	
e	0·991121 $+·0000028\,d\nu$	0·990177	
Period ...	1143 years (for $d\nu = -40''$)	1001 years.	

It will be seen that the adoption of the value $-40''$ for $d\nu$ will bring the two orbits into almost perfect accord, which is satisfactory. There would seem to be little room for doubt as to the ellipticity of the orbit. Mr. Peck proposes to examine the planetary perturbations previous to discovery, to see if they will explain the divergence from a parabola. A. C. D. C.

MINOR PLANET NOTES.—According to Prof. Wendell, of Harvard Observatory, the variation in light of Eros had become zero on May 8. This rather favours the hypothesis of the planet being double or dumb-bell-shaped, and that the Earth is no longer in the plane of motion. Were the variation due to markings on the surface it could scarcely have sunk so soon to zero. 'Harvard Circular' No. 58 deals with this question. It is pointed out that there are four factors which will have to be allowed for in determining the brightness at any moment, viz.:—(1) velocity of light, (2) distance of Sun and Earth, (3) phase, (4) direction of axis of rotation. The last can only be determined by a long series of observations extending over a large angle. As a first rough approximation the equator of Eros is assumed to be parallel to ours, and the consequences of this assumption are deduced.

The photographs taken in 1893, 1894, 1896 have been examined for light-variation. Some of the plates had a long exposure (1^h or more), and Eros had trailed considerably; this would enable any variation in light during the exposure to be detected (such variation might, however, arise from atmospheric causes). The range seems to have been very small in 1893 and January 1894. A maximum is indicated on Julian day 2412865·622 (February 5, 1894).

In 1896 plates show larger variations of light. In June of that year the variation seems to have been at least 1 mag. Maxima are given at Julian dates 2413656·890, 2413715·702, 2413740·803, 2413741·561. The photometric observations made in 1898 give the means of investigating the variation at that date. A discussion of these is promised in a future Circular. A. C. D. C.

THE SUN-SPOT GROUP OF MAY 19–JUNE 1.—Since November 27, 1900, the solar surface has been almost entirely clear of spots and faculæ, the only noticeable group of spots being one composed of small members which was observed from March 3 to March 11. But after a period of almost exactly six months quiescence, the solar faces displayed a renewal of at least partial activity in the shape of a fine group of spots, visible to the unaided eye, which crossed the visible disc between May 19 and June 1. The group was born very close to the E. limb on the invisible side of the Sun, being probably about two or three days old when first observed at Stonyhurst on May 20th. The heliographic position of the spot-group was then approximately longitude 160° and latitude 9° N. On this date the group was already approaching the phase when two spots predominate, or Type II. of a recently proposed nomenclature. On May 22nd the space between the two main spots of the group was partially filled by small spots and penumbral patches, representing Type III., which generally occurs from five to seven days after the birth of a spot-group. The leader had now become more regular and defined in outline, while the following main spot was growing rapidly, being composed of several large detached fragments. Seemingly it attained its greatest area on May 24th. The group was central on the solar disc on May 25th, when its greatest length extended about 12° in longitude. The co-ordinates of the centres of the two main spots were now longitude 165°·5 and 157°·0 respectively, latitude of both about +8°·0. In accordance with the usual mode of procedure, the leader had advanced in longitude as the rearmost spot developed. This last spot then began to break up with a decrease in total area, the greatest change being observed between the dates May 27–29. It was on this last date composed of a few small scattered fragments. The leader had now become a spot of round regular outline, but without any well-developed black nucleus. It may, however, possibly return again at the E. limb about June 14th as a small single spot, unless indeed, as sometimes happens, a fresh outburst should take place in the same region of disturbance. When the spots were observed by means of a Thorp polarizing solar eyepiece on the 15-inch equatorial, the dark nuclei in the umbræ, particularly in the leading spot, were well seen, and they appeared to be rents in a faculous veil which was spread over the umbræ. Beautiful bright tongues extending from the edges of the several umbræ, but not quite bridging them over, were also a feature of the spots, as well as a faculous cloud of less vivid brilliancy, presumably floating above the umbræ.

Visually it would be judged that the group was not very dense, as there was no formation of a single compact black umbra. In the spectrum too of the spots the general absorption even of the leading spot was only moderately dark. Hence, unless there be a fresh outburst in the same locality, we would hazard the opinion that the group will not be a long-lived one. With regard to the selective absorption, there was no change from that which seemingly steadily prevails in all spots, at least in the red end of the spectrum between the lines B and D. We have here at minimum a spot showing identically the same spectrum as a spot at maximum, the most widened lines being faint Fraunhofer lines, due for the most part to vanadium. Two sodium lines and a nickel line were also greatly widened, as was a line at the tail of the α group, which has been noted before as a feature of this region of the sun-spot spectrum, and which is exceedingly near, if not coincident with, a line of oxygen. The group attained its greatest area of about 700 millionths of the Sun's visible hemisphere on May 24th.

A. L. C.

NOVA PERSEI.—The following table continues that on page 207 of last month's number, and replaces some of the figures there given. The magnitudes are, as before, simple means of results by various observers, the numbers which go to make up each being shown by the suffix. On April 24 the magnitudes by different observers range from 5·0 to 6·0, and one observer notes a rapid change throughout the evening. A glance at the figures shows a sudden brightening about every five days.

	m.		m.		m.
April 6....	$4{\cdot}4_1$	April 18....	$4{\cdot}2_3$	April 28....	$5{\cdot}4_2$
7....	..	19....	$5{\cdot}3_4$	29....	$5{\cdot}8_4$
8....	$4{\cdot}6_1$	20....	$5{\cdot}6_5$	30....	..
9....	$4{\cdot}5_2$	21....	$5{\cdot}9_5$	May 1....	$5{\cdot}3_1$
10....	$5{\cdot}4_3$	22....	$5{\cdot}7_6$	2....	$4{\cdot}5_2$
11....	$5{\cdot}7_1$	23....	$4{\cdot}3_6$	3....	$5{\cdot}7_4$
12....	$5{\cdot}0_5$	24....	$\left\{\begin{array}{l}5{\cdot}0\\6{\cdot}0\end{array}\right.$	4....	$6{\cdot}1_2$
13....	$4{\cdot}4_3$			5 ...	$6{\cdot}2_1$
14....	$5{\cdot}4_3$	25....	$5{\cdot}9_5$	6....	$6{\cdot}1_2$
15....	$5{\cdot}6_4$	26....	$5{\cdot}8_2$	7....	..
16....	$5{\cdot}3_3$	27....	$4{\cdot}3_1$	8....	$4{\cdot}6_1$
17....	$4{\cdot}8_2$				

THE FRENCH ECLIPSE EXPEDITIONS.—Under the authority of the Minister of Public Instruction, the eclipse was observed by two French expeditions organized by M. Janssen, who publishes the first results, received by code telegrams, in the *Comptes Rendus* for May 20. Count de La Baume-Pluvinel, an experienced eclipse observer, went to Sumatra, his programme being to photograph the corona, to obtain evidence as to its rotation, and to find out whether there were any more or less marked dark Fraunhofer lines in the bright corona, and hence to deduce information as to the calorific radiation of the same. The telegraphic report states that, except for some slight cloud, the observations have been successfully carried out, and speaking guardedly, as the information is as yet so meagre, M. Janssen states that the rotation of the corona has not been determined, and that the presence of dark Fraunhofer lines in the corona has not been made out, which he thinks is consistent with the fact that the sun-spots are at minimum; for, he goes on to say, "it is at the epochs of maximum that the vapours of the solar globe raise themselves in the coronal atmosphere and permit those phenomena of reflexion of photospheric light marked by the presence of Fraunhofer rays."

The other expedition, under the charge of Dr. Binot, went to the island of Reunion; but about this party, except for a telegram stating that the weather was magnificent, there is no information.

THE WEATHER IN MAY.—Fresh north-easterly winds prevailed until the 25th, and the weather remained generally cold, although not exceptionally so, the mean temperature for the eighteen days May 4 to 21, most of which ranged below the average, was 50°·4, which is lower than the average by 1°·8. Rain fell on 4 days (May 7 to 10) to the total amount of 1·56 inch, an inch of which fell on the 9th alone. A prolonged drought set in on the 11th, continuing for nineteen days, accompanied by brilliant sunny weather. From the 12th to the 29th the sunshine amounted to 187 hours out of a possible amount of 284 hours, and yielded the high percentage of 66. On May 15, 22, 23, and 24 the percentages amounted to 91, 89, 89, and 90 respectively. The weather became somewhat warmer after the 22nd, and very much warmer after the 28th, culminating in a maximum temperature of 83°·2 on the 29th. A heavy thunder-shower on the morning of the 30th ended the drought, and initiated cooler weather.

THE ROYAL SOCIETY SOIRÉE.—The astronomical exhibits at Burlington House on May 8 related mainly to the new star in Perseus. Mr. McClean, Father Sidgreaves, and Sir Norman Lockyer all showed photographs of its spectrum, and the latter also showed a diagram giving its variation of magnitude. The astrographic micrometer of the Cambridge Observatory was exhibited by Mr. Hinks. There will be no "Ladies' Conversazione" this year, in consequence of the death of Queen Victoria.

THE Society of Mechanical Engineers has of itself little connection with astronomy, but this year Mr. W. H. Maw, Treasurer of the Royal Astronomical Society, happens to be its President. For this reason the fact is recorded here that Mr. Maw received the Society and other of his friends at a conversazione in the superb rooms of the Mechanical Engineers at Storey's Gate on the evening of May 17, and entertained them with a delightful programme.

Capt. Wm. Noble writes to point out an obvious mistake in our last number. In the report of the R. A. S. Meeting it was implied that Prof. Burnham found a star to be double in the year 1851, which was many years before he began to observe. On p. 186, line 14, *for* "15" *read* "53."

Mr. F. W. Dyson, M.A., Chief Assistant in the Royal Observatory, Greenwich, has been recommended by the Council for election as a Fellow of the Royal Society. We offer our best congratulations. Mr. Dyson is the only astronomer in the list this year.

Sir William Huggins has been awarded the Henry Draper Medal by the U.S. National Academy of Sciences.

The next Meeting of the Royal Astronomical Society will be on Friday, June 14; of the British Astronomical Association on Wednesday, June 26.

From an Oxford Note-Book.

The news from Sumatra seems rather sad, but it is as yet scanty, and we must not give up hope. Mauritius, on the other hand, sends us distinctly cheery intelligence, and the eclipse has clearly not been a complete failure. On such occasions of varying success we view the misfortunes of our friends with mixed feelings. I have told before in these notes the story of the Russian totally cloudy eclipse of 1887: how several dejected astronomers met in the big restaurant in Moscow and gradually brightened up as they compared notes and found that the disappointment had been universal, until there came in a man who announced with a beaming face that he had seen the corona through clouds, which plunged the others back into gloom. Astronomers are, after all, but human, and a cloudy eclipse tries human nature pretty severely. In a copy of the *Figaro* sent me the other day by a kind correspondent who happened to be in Paris on May 21 there is a delightful manifestation of humanity which I hope our French friends will forgive me for quoting. It is really too good to be lost; and I wish I had room for the whole of it:—

> Elle vient de fournir à la science française une occasion nouvelle d'affirmer son indéniable supériorité. Vous vous souvenez que l'éclipse de soleil annoncée pour samedi dernier, entre deux et trois heures du matin, devait être visible dans l'île de Sumatra? Deux missions scientifiques étaient parties, l'une

> anglaise, sous la direction de l'éminent professeur Todd: l'autre française, confiée à M. de La Baume-Pluvinel, un des plus distingués élèves de M. Janssen. Or il parait—lisez plutôt de dépêches du *New York Herald*—que les résultats obtenus par les savants anglais seraient insignifiants, alors que, grâce à des instructions précises et aux multiples précautions prises par lui, M. de La Baume-Pluvinel s'en revient riche d'observations intéressantes.

The paragraphist goes on to state that he sought an interview with M. Janssen, to hear all about this new French triumph, and M. Janssen showed him the actual cipher telegram — "*Ours* 23689"—received from M. de La Baume-Pluvinel, and translated it as follows:—

> *Ours* signifie en somme "Mon observation a réussi." 23689 donne les détails de l'observation—de cette réelle victoire! Vous savez que les Anglais ont télégraphié "La couronne du soleil est restée invisible." Et bien! alors que les Anglais, mal installés je pense, n'ont rien vu, voilà ce qu'a apercu M. de La Baume-Pluvinel. Je décompose le 23689. "Le programme d'observation a été rempli. Malgré de légers nuages l'étude a été possible de la *Couronne* pendant l'éclipse. Des raies noires peu sensibles, en accord avec le minimum des taches solaires, ont été constatées. . . ." Ceci encore: "si la rotation de la couronne n'était pas visible, cette couronne avait, du moins, une action calorifique des plus appréciables." . . Comprenez-vous? Je m'applaudis donc d'avoir contribué à faire obtenir à M. de La Baume-Pluvinel—il a entrepris le voyage à ses frais, d'ailleurs, notez-le!—la mission du ministère de l'instruction publique qui va offrir d'aussi précieux résultats. C'est à proprement parler, un gros succès pour la science française.

And the interviewer did not lose his chance, but promptly replied to his veteran victim:—

> Un succès dont une bonne part vous revient, monsieur!

The mention of cloudy eclipses reminds me of a clipping from the *English Mechanic*, which has been lying idle more than a year —itself an excerpt from an article in another paper, as stated in a footnote:—"From a clever paper—of course permeated by the badinage characteristic of its author (George Bernard Shaw)—in the first number of the *Humane Review*, an interesting and thoughtful quarterly, just issued, which we heartily commend to all (London: Ernest Bell, 6 York Street, W.C., 1*s*.)." The article is headed "Science and Common Sense," and its general purport may be gathered from the following extract:—

> On the whole, I like the invisible eclipse better than any of the other scientific devices, for it is salted with humour, and makes me long to go to the Observatory and hear the Astronomer keeping the telescope in a roar, so to speak, with witty conceits; whereas the mere thought of visiting a physiological laboratory, and watching a dull augur groping for oracles in the entrails of tormented dogs and rabbits, makes me sick and angry. As to the visible eclipse, the Astronomer generally places it at Spitzbergen. He did so some years ago, when a friend of mine actually went to Spitzbergen to see it. Of course, there was no such thing; but the simple remark that "unfortunately the day was cloudy" passed off everything as pleasantly as possible.

But a more interesting paragraph follows: more interesting because it seems to bear the stamp of truth. The surmise is somewhat risky, but do we not recognize our dear old friend the late Mr. Henry Perigal with his geometric chuck? Had "G. B. S." really the privilege of meeting him in his wanderings? I would have given much to be there.

> However, I think the astronomers overact their parts in the matter of the dimensions of space. When a man points to a star and tells me that it ceased to exist about the time of William the Conqueror and that I only see it now because it is so far away that the last ray of light that left it as it expired has not yet finished its journey to my eye, the magnitude of the lie, and the audacity of the implied pretension to have made the measurements, stuns more than it exhilarates. If to that savant were put the old questions, "Who hath laid the foundations of the Earth, if thou knowest, or who hath stretched the line on it?" how confidently would he reply that, as far as stretching the line was concerned, he was the man! But in this he exceeds my sense of the becoming. Years ago an ancient member of the Royal Society accosted me on the pier at Broadstairs as I was gazing at the Moon, and asked me how far off I supposed it was. I replied, "Probably about forty miles." To my surprise (for I had taken him for one of the usual billion and trillion megalomaniacs) he asked me very earnestly how I had arrived at that figure. I answered that I guessed it, just as I guessed the distance of the North Foreland light. "You are very nearly right," he said; "the true distance is thirty-seven miles." This seemed so reasonable that I asked him why Newton had not found it out. "Newton," he said, "was wrong in his method. If he had traced the orbit by means of a geometric chuck on a lathe, he would have found that the *calculated* orbit is a mere figment. *I* used the geometric chuck; and you may take it from me that the distance is thirty-seven miles." I did not understand the old gentleman's explanation, nor do not to this day; but as I do not understand the Newtonian method either, and its results are so much less plausible, I have ever since adopted the thirty-seven mile view. When speaking to astronomers, I now always offer them a limit of 150 miles, beyond which I decline to entertain any of their fairy-tales. The effect is most satisfactory. They become at once sane and friendly, dividing all their accustomed figures by a billion or so to humour me, and finding their own minds move much more comfortably in consequence.

Besides losing the eclipse to France, we seem likely to lose the comet to Germany if the *Daily Mail's* own Hamburg correspondent may be trusted. The following paragraph appeared on May 13, and was sent to me by one who had "been rather troubled by the inconsistencies of the newspaper reports, but managed to get on fairly well until he came to the enclosed, which proved altogether too much for him":—

> COMET NOT VISIBLE HERE.
>
> (FROM OUR OWN CORRESPONDENT.)
>
> Hamburg, Saturday, May 11.
>
> I have had a talk with Professor Krentz, of the famous observatory at Kiel.
>
> He informed me that the great southerly comet has not yet been seen on the Northern Hemisphere, and will probably never be observed in this country.
>
> According to the observations of Professor Krentz, made on the compass-

reckonings, the comet ought to be passing over the centre of Germany during the night of May 24–25 about three-quarters of an hour after sunset; then it will disappear.

But the twilight will not be dark enough for the presumably rather faint comet to be seen.

Some remarks made in these notes last month on the difficult question of duplicate publication have produced a veritable explosion from America. The language is so forcible, and some of it seems to me so likely to hit innocent people, that I thought of writing to the distinguished author for permission to modify some of the phrases. But on second thoughts it seemed better to let it go forward as it is. It bears all the characteristics of an indignant outburst, and it may be well for those against whom it is directed to know that this sort of view is taken (albeit an extreme view) of duplicate publication. To prevent misconception, however, I would beg leave to say that it is not my view; as stated in last month's notes, duplicate publication, if it can be managed without duplicate expense, seems to me a convenience and a clear gain. If I take in only one morning paper I like to have an important piece of news in all the papers, so that it is sure to be in my modest one. On the other hand, if I take in *all* the papers it is distinctly irritating to come across the same items continually, and this is rather the situation of the astronomer at present; he is practically bound to take in all the papers, or at least to go and read them at his Club, and he is liable, therefore, to get exasperated, as the following letter will show:—

Mr. Oxford Note-Book,—

Will you allow me to resent, good-naturedly, the charge which you have brought generally against Americans ("of whom I am which," as Artemus Ward would have said) in your remark on p. 215. Your words are: "they print the same paper in several journals, so that it may be widely read, whereas in Europe we have made it a point not to reprint." All I ask is that you amend by saying "a few of them." The ignorance of a few as to what good breeding requires ought not to be visited on a race. A man of science loses caste here for such practices quite as readily as elsewhere. It has been our hope for years that the small class of men, or the class of small men, addicted to them would finally get their eyes opened, and this hope has been realized largely. Few direct offences are now seen. The incorrigible ones, even, have been shamed out of the practice, and now resort to an indirect method, that of distributing articles broadcast in the form of circulars. This is expectorating out of the window on the heads of passers-by, instead of on the floor. Responsible editors have it in their power to rebuke this evil by refraining from reprinting such publications. Within a few weeks I have seen such things printed *in extenso* in five different journals. The thing is undignified to the extent of being charlatanic.

But another thing is worse and more exasperating. For a man to print a thing only once, in the ordinary self-respecting way, then to open some other publication and find it reproduced there without reference to its original source! He thus finds himself pilloried as having apparently committed the offence of which I have spoken, although absolutely innocent. This is *morally* wrong; let editors look to it.

Americus.

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A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

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April 24. From the papers by Mr. Innes and Mr. Lunt it appears that the faint tail was not seen in the early days of the comet's appearance, and it did not appear until May 6th or 7th.

The photographs of the comet were thrown on the screen. The comet had three tails, one especially well marked, the central one being fainter than either of the others.

The Chairman. I am not aware that the comet has been visible in the Northern Hemisphere, but it is a very curious object and strongly resembles in some features the comet of 1861.

Mr. Nevill. This comet, like a similar one seen in South Africa in 1882, appeared while I was on the high seas, so I had no opportunity of seeing it there; but I have had two letters from Natal giving an account of it. One of them is remarkable as giving a description of observations on the morning of Tuesday, April 23, whereas the comet was first seen at the Cape on Wednesday morning. It describes as a notable feature the transverse tail going off at an angle of 50 or 60 degrees, and states that it was well marked. That letter was sent to me before the observations at the Cape on May 4 and 5. I think it was dated April 30. It is an account of observations made in the north of Natal by two gentlemen, and there is confirmation of the fact that the comet was seen on Tuesday the 23rd. On that afternoon a telegram reached the Natal Observatory stating that a comet had been seen that morning. Unfortunately they were not able to see it the next morning on account of the cloudy weather. I think that shows that it must have been seen in Natal the day before it was seen at the Cape Observatory.

Prof. Forbes asked for the elements of the orbit which accompanied the paper. These were written on the board by Mr. Whittaker.

The Chairman. In returning our thanks to Sir David Gill this very interesting and valuable communication, I am sure we may also express our welcome to our old Fellow, Mr. Nevill.

Prof. Turner. There are two papers, one by *Sir David Gill* and one by myself, on the Oxford photographic determination of stellar parallax. I do not know that they are altogether suitable for reading verbally. Sir David Gill expressed his opinion, in his volume on stellar parallax, that the Oxford determinations made by Prof. Pritchard were not of proved value, and two months ago I gave some reasons for disagreeing with him. He has now replied to that paper, and I have again a rejoinder. There are one or two points which arise in the course of the controversy which are perhaps of general interest. One of the main points of theory on which Sir David Gill criticises Pritchard's procedure is that in making a determination of parallax Prof. Pritchard took a central star and four other stars equally distant from the central star in two directions at right angles. He could not get stars to fulfil those conditions exactly, but they were fulfilled approximately as far as was possible. He then measured the distance between two stars and found it varied on different nights, and he assumed that that was due to variation of scale-value on the photograph—a variation which may be due to increasing focal length of the telescope or, perhaps, to a change in the measuring-machine, &c. &c. The result is a composite one due to various causes, and as a whole, taking it as the measure of the scale-value, Prof. Pritchard used the distance between two stars to correct distances from each star to the central star. So far he is more or less in accordance with all other work—Sir D. Gill's work as well as his own. The point that Sir D. Gill criticises is that Prof. Pritchard used independent scale-values in the two directions at right angles. The parallaxes from all four stars were discussed independently. Sir D. Gill says "it is obvious" that the scale-value in two directions ought to be the same. That is where we differ. I do not think it is at all obvious, even if the instrument is behaving as nearly perfectly as may be expected. Possible causes for discordance are various. One is the distortion of the photographic film. We heard a great deal about that many years ago, chiefly from people who had not measured plates. No film is perfect, but it is astonishingly near perfection. Before people began to measure photographs this distortion of the film was a great terror. One way suggested for getting over it was to put the réseau on the plates. Now the réseau has been found very useful as a convenient means of measuring the plates, but I think the amount of distortion of the photographic film which has been eliminated by its use is astonishingly small. Another cause is, call it optical distortion if you like, or, going a point further, I have suggested that it may be called difference of curvature of the mirror in two directions. And here I am on ground which I do not feel very secure upon. I think we may all regard it as likely in the varying position of the mirror that there must be varying strain on the mirror, and it does not seem at all unlikely that a slight difference of curvature may result. Now assuming that, it seems to me very probable that what will result is some difference of scale-value in the two directions at right angles. The experience of those who have measured photographs suggests a possibility of that kind. The scale-value has not been found constant with great exactness. Many such cases of systematic difference have been found in practice. Until we have a little more experimental evidence we must take that as a possibility. Temperature is always a cause to be reckoned with. This must be looked after. Sir D. Gill makes it out that it is not a serious matter. Those are the chief points between us. Some of the figures given by Sir D. Gill, I think, call for revision. But these are the points of general interest—the photographic distortion and the curvature of the mirror.

Dr. Rambaut. I should like to ask whether Prof. Turner has considered the effect of a "tilt" in the plate; that is to say, a small error in setting the plate at right angles to the line joining the centre of the plate with the optical centre of the object-glass, or, what comes to the same thing, a shift of the foot of the perpendicular let fall from the centre of the object-glass upon the plate. Because if the tilt of the plate were at all considerable it would unquestionably bring about an error in the resulting position of the star whose place it was desired to obtain. I have no doubt that care was taken in the "squaring on" of these plates, but I also know by experience that a very small amount of tilt will bring about sensible errors.

Mr. Hinks. I think it has been found that the position of the plate when drying after development has some effect. If the plate is held in a vertical plane, the film has a tendency to stretch vertically. But if the plate is dried in a horizontal position, with the film up, it is liable to collect dust. The effect of any stretching of the film is, however, eliminated by the use of a réseau.

The Astronomer Royal. There is one point I will ask Prof. Turner—if he has considered the effect of the curvature in the plate itself, because, so far as I gather, the plate is assumed plane, whereas it is generally found that plates are more or less curved.

Prof. Turner. About the tilt of the plate Dr. Rambaut was quite right, and that was mentioned in my former paper as a possible cause of error, in which case the method adopted by Prof. Pritchard would be incorrect; but there is no controversy between Sir D. Gill and myself upon that point. I am glad to know that the effect of drying plates has already been tested, but, as Mr. Hinks remarks, if there is any distortion it is eliminated by developing the réseau along with the star-images. With regard to curvature of the plate, that is a point which, like some others, had not attracted attention in Pritchard's time, and no doubt is a source of error—but *accidental* error and not systematic. Against the possibility of such unforeseen errors Pritchard guarded himself generally in two ways. First he applied the method first to stars like 61 Cygni and Polaris, whose parallaxes were fairly well known, and showed that the method gave the proper result. Secondly, he took pairs of comparison stars at right angles. Now if, for instance, chromatic dispersion by the atmosphere (which is another source of error that has been lately discussed, but which, so far as actual experiments go, seems to have very little effect) had any effect on the observations, the effect would be different in these two directions and the parallaxes would not agree as they are found to do. On the whole I think Pritchard's work stands the test of such criticism as it has received remarkably well.

Lord Rosse, in the course of a paper on Observations of Nova Persei at Birr Castle, Parsonstown, said: During the time that the Nova Persei was visible opportunity was taken for observing that interesting object; but I happened to be in England at the time and no authentic information as to its existence reached us until it had passed its maximum brightness, and afterwards the extremely bad weather which prevailed much impeded observations.

For the same reason, however, the work of others was also fragmentary, and in the hope of filling gaps rather than of adding any important matter these notes and observations, made principally by Dr. Boeddicker, are sent in. Spectroscopic observations were not satisfactory, the instruments having been unused for some years and having deteriorated with time. With a direct-vision prism, however, placed between the eye and the eyepiece of the 3 ft. reflector the red line was quite unmistakable. Others in the more refrangible regions were at times satisfactorily seen, but it was not always easy with the means at our disposal to distinguish between a bright line and an absence of dark lines in a continuous spectrum. Through the whole time that the star was visible the prevailing easterly wind and the accompanying thin clouds generally diffused the star's light and destroyed definition of prism used without a slit. No photography was attempted.

Dr. Rambaut. We have been taking systematic observations of the new star at Oxford since the date of the last meeting of the Society and have found the variation of brightness to continue very much as during the previous month. The paper now presented contains the results of these observations of brightness and colour of the new star in Perseus. With the exception of two observations on May 12 and 14, all observations were made with telescopic aid. After May 18 the star could no longer be followed with the 10-inch Barclay equatorial, and from that date observations were made with the 7½-inch heliometer. The observations continue to indicate the relationship already pointed out between the colour and magnitude of the star, the redness of the star increasing as its brightness diminished. The period of oscillation in brightness continues to vary. Between May 11 and May 24 there were two periods of 4 or 5 days followed by a three days' period. The observations have been interrupted by cloudy weather since the latter date, and since the close of that 3-day period we have not been able to determine what the period of observation is. The brightness seems to vary through about a magnitude and a half, while the mean magnitude about which the oscillation takes place remained unaltered since April 20.

The second paper is on the magnitude of Nova Aurigæ in 1899 and 1900, with the mean annual results from 1892 to 1900. The observations here given are in continuation of those published in the *Monthly Notices* at various dates, to which references are given in the paper. A chart was published in one of these earlier papers giving a number of comparison stars. We have continued to use these stars for comparison and they are referred to by the letters given in the chart.

On 1899 April 1 the magnitude was determined by Mr. McClellan as 13·5, and on the 10th of the same month it was determined by Mr. Robinson as 13·3. Last year the individual observations of Oct. 4th, Nov. 17th, and Dec. 13th came out 14·1, 13·9, and 13·8. At the beginning of the year 1892 it was of magnitude 4·5; and then between Feb. 3 and March 31 it fell to 14th magnitude.

It subsequently brightened, and between Sept. 8 and the end of the year it varied between 9·5 and 9·8. The mean results for the subsequent years were 9·7, 9·7, 9·7, and in 1896 we do not seem to have got it at Oxford. In 1897 it was 11·4, in 1898 it was 12·0, in 1899 it was 13·4, and last year 13·9.

Mr. Whittaker read a note on Nova Persei by *Mr. Stanley Williams.* Mr. Williams's results had been compared with those of Dr. Rambaut and there was a very good agreement. Mr. Williams's observations extended from May 12 to May 24; the evening observations were then discontinued, and on June 6 a new series of morning observations was begun; he found two periods of 5 days each in the interval between May 12 and May 24 just as Dr. Rambaut did.

Prof. Turner said that Mr. Bellamy had sent a slide, taken when the Nova was below the pole at an altitude of 8 degrees. The point of interest was the apparent brilliancy of the Nova, although it appeared faint to the eye.

The Chairman. Looking at Dr. Rambaut's observations, I notice that he makes a difference between June 6 and June 8 of a magnitude and a half, whereas Mr. Williams makes only eight-tenths of a mag. between the same dates. I would ask Dr. Rambaut whether a photometer of any kind was used at the Radcliffe Observatory.

Dr. Rambaut. No; our measures were made by eye-estimation, but I have every confidence in them. It is a matter of surprise the degree of accuracy obtainable in this mode of observation. I may mention as an illustration of this fact that we discovered an error in the brightness of two of our comparison stars (κ and γ Persei) as given in the Harvard Photometry, from the systematic differences which appeared in the brightness of the Nova whenever these stars were used. By comparing these two with the other comparison stars we deduced corrections to their brightness as given in the Harvard Photometry. On looking up the Revision of the Harvard Photometry we found a close corroboration of the corrections to which we had been led.

In reply to a Fellow, *Mr. Whittaker* said: Mr. Williams's observations were made in the morning after midnight, and there is a difference of a few hours from the time of Dr. Rambaut's.

Dr. Rambaut. At the time when the star is varying most rapidly a few hours would make a very great difference.

Mr. Whittaker. As a matter of fact, the difference is 4 hours.

Mr. Crommelin. Do the recent observations of Nova Aurigæ at Oxford indicate that it is now of the nature of a star or a nebula? There had been a keen controversy on this point in 1893. It would be interesting also to know whether observations of the position of the Nova were made, and if so, whether they showed any trace of proper motion since 1892. I believe Prof. Barnard suspected a slight proper motion from micrometric comparisons with neighbouring stars.

Dr. Rambaut. The observations of the Nova Aurigæ were not made by myself, and only some of those of the Nova Persei, but one of the observers of Nova Aurigæ has noted it as being nebulous and—I speak from memory and may make a mistake—has suspected it of having a stellar nucleus. We have not recently made any determination of its position.

Mr. J. C. W. Herschel. The paper that I now present to the Society is on the Lyrids, 1901, April 12–22, observed at Cambridge. I give details as to how I treated my observations. Taken all together they give an ill-defined radiant area about 10° by 5° between 268° +28° and 278° +37°.

I have tried to show that this area contains six distinct radiant-points. With these I compare Mr. Denning's similar group of 20 meteors, which he kindly sent me and said I might use, and I found that they split up into five radiant-points, three of which agree with my own. (Blackboard.) Out of both groups together five points agree with "apparent points of condensation derived" by him "from various Austrian observations in the years 1863–73." They lie in nearly a straight line. A row of radiant-points like this may well give the impression of a radiant in motion. But I found this year that the radiant at 278° +37°, which should in that case have been the last to appear, was in evidence on the 12th, and right up to the 22nd, and all the others appeared together on the 20th and 21st. This points, not to one radiant in motion, but to an apparently connected group of stationary radiants.

Prof. Turner. This is a most interesting view of radiants and the possibility of the motion of the radiants that Mr. Herschel is putting forward, that we may have a series of independent radiants, and possible confusion between them might lead to the idea of a moving radiant. I think I gather that Mr. Herschel is attacking one of the very few radiants which Mr. Denning admits as possibly moving; so if he demolishes that there will be nothing left but the stationary radiants for which Mr. Denning has so pluckily contended. It is much bound up with the problem as to what is the accuracy of the determination of the radiant, and we are getting some light on that point when we find that Mr. Herschel can put down radiants which are so closely confirmed by Mr. Denning. So we do seem to get independent testimony as to the existence of separate radiants even very close together. I should like to mention a circumstance in this connection which may well excite a little alarm among astronomers as to the value of independent testimony of this kind. Prof. Karl Pearson has read a paper to the Royal Society, in which he sounds a note of warning, as to personal equation observations. He has found the remarkable result that observers seem to vary in sympathy from causes which are not obvious and which are very hard indeed to detect. He got two people independently to bisect at sight a lot of lines, and the distance from the actual middle point was measured. The observations were put down in actual sequence, and it was found that the personal equation for two observers varied about its mean value in such a way that the two curves were very nearly similar. The observations were not made at the same time, but in the same order. In Prof. Pearson's language, they are correlated, and the existence of such correlation is in his opinion much more widespread than we may at first think. It strikes a blow at the whole independence of testimony—the belief that when we get two witnesses to the same phenomenon we can accept the phenomena as confirmed.

Prof. Alex. Herschel. I should like to say a few words on the set of Lyrid centres which my nephew, Mr. J. Herschel, has described. The nights of the 18th–23rd April last were exceptionally favourable ones for Lyrid observations, and numerous notes of the meteors were recorded by members of the British Astronomical Association, the sky having been very clear, the Moon absent, and the shower a rather abundant one on the maximum night of April 21st. Good determinations, therefore, like those described, of the Lyrids' centres on those nights are certainly of very particular interest and value. Among several lists, including that by my nephew at Cambridge, and that which Mr. Denning at Bristol kindly sent to me, I have projected a list of about 60 tracks by Mr. C. L. Brook at Meltham, near Huddersfield, together with such of my own mapped paths as come on the above nights from the direction of Lyra and its neighbourhood. These amount to about forty paths, and Mr. Brook's list supplied about an equal number of swift paths coming from north-easterly centres in the neighbourhood of Lyra. On sorting these under the known contemporary radiant-points in that quarter of the Lyrid shower, it was soon found that the really indubitable Lyrids reduced themselves to about fifteen or sixteen in each list, the rest of the swift-flighted tracks, including at Slough all (except one doubtful Lyrid or α-Aquilid) that left persistent streaks, being clearly derivable from the two known centres in Cerberus, near Bode's stars C and K, from near α and η Aquila, and from one or more radiants, principally near ζ in Cygnus.

The result of the thirty Lyrid paths' projection does not resemble much what Mr. J. Herschel's figure and the description given of it in his paper show; it seems to furnish only two mean centre points, at about $266° +30°$ and $271° +30°$, the latter being chiefly marked on April 21, and then very sharply defined, while the former point ceased to be traceable after April 20; hence if a real motion with the time was thus indicated in the radiant-point's position, it seemed to be onwards in R.A., about 5° in the four nights of April 18–21, along a parallel of declination. Certainty is not easily attainable among the very mixed flights of the Lyrid meteor-epoch, but a rather more definitely distinct result than usual seemed to be obtained this year by excluding from the Lyrid meteors' summary all paths which could more or less certainly be traced back to the swifter simultaneous streams from Cerberus, Aquila, and Cygnus *.

Mr. J. C. W. Herschel. With reference to Professor Turner's warning (illustrated by the example he gives), that one must be careful how one accepts coincidences as confirmation, I think my results are a *real* agreement with Mr. Denning's because they *both* agree with the Austrian observations taken under quite different conditions, the combined results, too, of several years. My principal radiant is 278° +37°, and Mr. Denning's is 270° +33°: why they should be different I do not know, but the disagreement tells against the supposition of an identical outside cause giving a falsely identical result.

As to their being in a row, it is just possible that this may be a spreading action in process due to perturbation of some kind, such as Prof. Turner suggests as an explanation of stationary radiants; but if so, I do not see how it can be due to the Earth, as it is not parallel to the ecliptic, but more nearly to the Milky Way.

Mr. M. Horner introduced a paper on some solar observations by saying that the Society seldom heard much about the prominences which were continually occurring on the Sun, and he could only ascribe this fact to the dislike of observers with smaller instruments to disturb the perfect adjustment of such telescopes by the weight of a heavy spectroscope. It was always interesting to observe the Sun when near an eclipse, and on several occasions he had been trying to observe the Sun at such periods to determine two objects: first, as to whether the eclipse observations agreed with those made in full sunlight; and, secondly, to trace the connection, if any, between the line 1474 and extensions of corona.

Mr. Evershed. The line 1474 is generally thought to be not a coronal line, but is due to iron, and reversals of this line observed at certain periods on the Sun's limb probably have no connection with the coronal streamers.

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* [It seems difficult to account for these concomitant streams reaching a combined maximum at the same time with the short-lived shower of Lyrids, producing all together a considerable meteor-frequency for three or four nights; but perhaps in that connection, a conjectured early stage in the history of Comet I. 1861, the supposed parent comet of the Lyrid meteors, may be worth recalling, that it is one among some other long-timed elliptic comets which, in his valuable and suggestive paper in *The Observatory* (1880), Professor George Forbes regarded as having been originally introduced into the solar system by an encounter with an extra-Neptunian planet whose probable distance from the Sun and present apparent place were there calculated. Besides the original disturbance which at some remote age appears to have probably deflected it into the Earth's neighbourhood, it might even be not an impossible conjecture that some particles of that parent comet may have also been deflected by the Earth itself into new orbits during exceedingly long ages (to judge by its dispersion) of this now apparently continuous meteor-stream's far-ranging successive revolutions and often repeated subjections to the Earth's attraction.—*Note*, A. S. H.]

Mr. Horner. That is precisely what I wish to determine, as there is no other line at all near which might cause confusion. I think that the reason of my seeing the reversal so easily is because I lose no light by reflections and passage through a train of prisms which most observers find necessary.

Mr. Thackeray read a paper by *Mr. D. P. Todd* on a modified form of occulter for regulating the exposure of the Sun's corona at varying distances from the Moon's limb according to its actinic intensity.

Mr. Knobel. I should like very much to see this apparatus, and also the application of it. I have just had a card from Mr. Todd, written before the eclipse, and he was very hopeful of good results. He was going to try both dry and wet plates.

The Astronomer Royal. I am very interested in this device of Mr. Todd's. Mr. Dyson has taken out to Sumatra an arrangement on Mr. Burckhalter's plan. The difficulty with Mr. Todd's arrangement seems to be in working with a suitable exposing shutter. During eclipse work it is pretty clear that it is not safe to have an ordinary plate-carrier, in which you have to draw out a slide which may stick and perhaps cause displacement of the telescope tube. The great object is to change the plates as quickly as possible, and Dr. Common proposed to have a hinged shutter, but it seems difficult to have an occulter on Mr. Todd's plan with that arrangement.

Mr. Whittaker then read a paper by *Mr. A. W. Roberts* on secular variation in the period of R Carinæ. This star was discovered to be variable in 1871; but observations were to be found which go back a very long time, including one by Lacaille in 1752, which had proved to be of great value in Mr. Roberts's discussion. A very interesting circumstance had come to light. The period was 309·3 days as a mean; but if they took the estimated dates of maximum as formed on that supposition, and compared them with the true dates of maximum, they got discrepancies which were plotted out in a diagram sent by Mr. Roberts. The representative points of these discrepancies lie approximately on a sine-curve, and from this an expression for the periodicity of magnitude can be deduced. Mr. Roberts pointed out that that was, however, only one of several formulæ which represented the state of things with a tolerable degree of accuracy, and the observations of the next ten years would enable one to determine with certainty which formula really represented the permanent truth.

Mr. Whittaker said that there were two papers from *Mr. Gledhill*, one on measures of double stars made at Mr. E. Crossley's observatory, Bermerside, Halifax, and one on physical observations of Mars made at the same observatory. There was also a paper by *Mr. W. Ellis* on sun-spots and magnetic disturbances. Mr. Ellis's paper discussed some of the questions raised by Father Sidgreaves in his paper on the subject in the Mem. R. A. S., and in particular the eleven-year period. Mr. Ellis remarked that there was a seasonal period in various phenomena, and he discussed how far this seasonal variation was similar in the different phenomena. He found a very remarkable degree of concurrence between them.

Dr. Downing had also sent a paper on corrections to reduce the revised Madras catalogue of stars for 1835·0 to the fundamental catalogue of Auwers.

The following papers were announced and partly read :—

J. Gledhill. "Observations of Mars made at Mr. Edward Crossley's Observatory, Bermerside, Halifax, during the Opposition of 1900–01."

D. P. Todd. "A Modified Form of Revolving Occulter for adapting the Exposure of the Sun's Corona to its Actinic Intensity at all Distances from the Moon's Limb."

Sir D. Gill. "The Oxford Determinations of Stellar Parallax: Reply to Prof. Turner."

W. Ellis. "Sun-spots and Magnetic Disturbance."

The Earl of Rosse. "Observations of Nova Persei made at Birr Castle, Parsonstown."

A. W. Roberts. "Secular Variation in the Period of R Carinæ."

Sir D. Gill. "The Great Comet of 1901, as observed at the Royal Observatory, Cape of Good Hope."

H. H. Turner. "The Oxford Determinations of Stellar Parallax: Further Reply to Sir D. Gill."

J. Gledhill. "Measures of Double Stars made at Mr. E. Crossley's Observatory, Bermerside, Halifax."

A. M. W. Downing. "Corrections to Reduce the Revised Madras Catalogue of Stars for 1835·0 to the Fundamental Catalogue of Auwers."

J. C. W. Herschel. "The Lyrids, 1901 April, observed at Cambridge."

A. Stanley Williams. "Further Observations of the New Star in Perseus (4)."

M. Horner. "Observations of the Sun during May 17, 18, and 20 at Mells, Frome, Somerset."

Radcliffe Observatory, Oxford. "Estimations of Magnitude of Nova Aurigæ in 1899 and 1900, with Mean Results for the Years 1892–1900."

Radcliffe Observatory, Oxford. "Further Observations of the New Star in Perseus."

The following gentlemen were elected Fellows of the Society :—

F. W. Crook, B.A., 4 Overcliff, Gravesend.
Frank Lowman, B.A., St. John's College, Battersea, S.W.
Charles Nielsen, 15 Cliff Terrace, Hartlepool.

The following Candidate was proposed for election as a Fellow of the Society :—

W. J. Greenstreet, M.A., Head-Master, Marling School, Stroud, Gloucestershire (proposed by C. T. Whitmell).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The seventh ordinary Meeting of the eleventh Session of the British Astronomical Association was held on Wednesday, May 29, at Sion College, *Mr. G. M. Seabroke*, the President, in the Chair.

The names of four Candidates for membership were read and passed for suspension, and the election by the Council of three new Members was confirmed.

Mr. Crommelin read *Col. Markwick's* third report on Nova Persei. The writer said that all the observations of magnitude of the star for April had been plotted, and the diagrams (which were handed round) would convince anyone that the most remarkable fluctuations in light had been going on all through the month. On March 31 the star was fading, having reached a maximum probably about noon of that day. It fell probably to a minimum at noon of April 2, but he could not be certain of this, as no observations were made either on April 2 or 3. A maximum was reached (about 3·8) on April 4 at noon. Then a gradual declension took place till a minimum of about 5·4 was reached on April 7, 6^h. A sharp rise then occurred to 3·9, reached on April 8, 20^h. This particular swing or undulation of April 4–8 was characteristic of several that followed, the increase of light being gradual, while declension was sharp. Towards the end of the month this seemed to change, the maximum being represented by a sharp peak or crest symmetrically situated as regarded ascent or descent on either side. Col. Markwick then referred to the difficulties in the way of getting at the minor fluctuations with any degree of accuracy, the observations being practically limited to four hours out of the twenty-four. The star was now immersed in the mists and twilight of the northern horizon, and observations would be more or less impossible for the amateur in ordinary hours until the summer was over, the twilight began to fade, and Nova swung upward in the N.E. sky. As regarded colour, not many notes had been made, the fading of the star's light, combined with its unfavourable position in the sky, rendering such determinations difficult. On the whole, the star appeared to have maintained its ruddy character, although it was not so marked as in the brighter stages.

Mr. Crommelin exhibited, by means of the lantern, some photographs of the Nova taken at Allahabad, in India, by Mr. H. G. Tomkins. The first of these was of interest as being the first photograph taken of the star (taken on February 22, at 10^h

local time). Mr. Tomkins was experimenting with a magic-lantern lens of 2-in. aperture, and had happened to choose Perseus for the purpose. Hearing afterwards of the discovery of the Nova he examined the plate, and found the star on the negative.

The President remarked that a good deal had been said about the magnitude of the Nova, the magnitudes being given in fractions on the Harvard College scale. It would be of interest to him, and he thought perhaps to others also, if some Member would explain how the comparisons were made.

Mr. J. W. L. Child said the figures given as the magnitude were the mean of the estimates of the number of steps between the comparison stars and the variable, each observer having, of course, his own personal equation, so that a step varied from about ·10 to ·14 magnitude. The variable was then entered as being so many steps greater or less in magnitude than the comparison star, and the mean of all the deduced magnitudes was taken as the value for the time of observation.

The President. You have a number of well-known comparison stars of known magnitudes, and you compare the Nova, or whatever star it may be, with it, as being so much greater or less than certain known stars; and those given stars are from the Harvard determination—hence the name?

Mr. Child. Yes.

Mr. F. W. Backhouse said he was surprised to notice, from the diagram accompanying Col. Markwick's paper, how great the variations had continued to be lately, as compared with what he himself had noticed. He could only suppose that he had missed seeing the Nova at its maximum and minimum.

Mr. Petrie read the Third Interim Report of the Variable Star Section, giving the results obtained since the presentation of the previous report.

The President, referring to this report, said it bore evidence of the very great trouble Col. Markwick took in connection with the section. The variability of stars was a very interesting subject. The reasons for such variation were obscure, and it was hoped that the many observations which could be made by people without very large instrumental means might some day or other be of great value.

Three slides illustrating Mr. G. J. Newbegin's observatory at Thorpe St. Andrew, Norwich, were then thrown upon the sheet, and the construction and equipment of the observatory described by Mr. Newbegin.

Mr. Goodacre showed a lantern-slide of a photograph of a portion of the lunar surface recently taken at Yerkes Observatory under the direction of Prof. Hale. The slide was made from a photograph just presented to the Royal Astronomical Society, and showed the Mountain Ring Theophilus and surrounding districts. Full particulars of the method by which this photograph had been taken had not yet arrived; but it was understood that the full aperture of the 40-inch refractor was used, the image being received on a "Cromer" isochromatic plate after passing through a yellowish-green screen; length of exposure about half a second. Mr. Goodacre considered that this photograph was distinctly the best of any yet taken of the Moon's surface, being superior to the Paris or Lick negatives in sharpness and amount of detail shown, and in the accuracy of the respective positions of these details. Hitherto, on comparing photographs and maps, it had always been found that less detail was shown in the photograph than on a good map like Schmidt's; but in this case the photograph showed in some ways more than the map.

Mr. Crommelin made a statement of the facts relating to the discovery and subsequent observations of Comet a, 1901.

Mr. Crommelin then made a statement concerning the recent total eclipse of the Sun. He said that all the observers were agreed that the corona was of sun-spot minimum type. As seen from Mauritius, the Sun's equator and consequently the long streamers of the corona were practically vertical, thus giving it the rather unusual appearance of standing on end. The coronal light was traced all round for 2½ radii, and in the equatorial extensions for 6 radii. The Mauritius parties had better observing conditions than those in Sumatra, where all the observations were hampered by a good deal of cloud. The American party had unfortunately scarcely any results. Both Mr. Dyson and Mr. Newall, however, obtained useful results in spite of the cloud hampering them.

ROYAL METEOROLOGICAL SOCIETY.

The closing Meeting of this Society for the present Session was held on Wednesday afternoon, June 19, at 70 Victoria Street, Westminster, *Mr. W. H. Dines, B.A.*, President, in the Chair.

A paper by *Mr. H. Helm Clayton*, of the Blue Hill Observatory, U.S.A., on "The Eclipse Cyclone, the Diurnal Cyclones, and the Cyclones and Anti-Cyclones of Temperate Latitudes," was read by the Secretary. The author has discussed the meteorological observations made along the path of the total solar eclipse in the United States on May 28, 1900, and also those made during three previous eclipses. He finds that a cyclone followed in the wake of the eclipse, though the changes are very minute and feeble, the fall of temperature developing a cold-air cyclone in an astonishingly short time, with all the peculiar circulation of winds and distribution of pressure which constitute such a cyclone.

A paper by *Mr. F. Napier Denison*, of Victoria, British Columbia, on "The Seismograph as a Sensitive Barometer," was also read by the Secretary. A Milne seismograph was installed in 1898 at the Meteorological Office, Victoria, B.C., and the author has since that time compared its movements with the changes of atmospheric pressure recorded by his aerograph. He finds that when the barometric pressure is high over the Pacific slope from British Columbia southward to California, while off the Pacific coast the barometer is comparatively low, the horizontal pendulum of the seismograph tends to move towards the eastward. This movement appears to be due to a distortion of the Earth's surface, caused by the heavier air over the Pacific slope depressing the underlying land-surface below its normal position; while, on the other hand, the comparatively light air over the adjacent ocean tends to allow the sea and earth beneath to rise above its normal level. It has been found that when an extensive storm-area is approaching from the westward, and often 18 to 24 hours before the local barometer begins to fall, the pendulum of the seismograph swings steadily to the eastward, completely masking any diurnal fluctuations that might have existed, as the storm-area approaches; and in the event of it being followed by an important high area, the pendulum will begin to swing towards the westward before it is possible to ascertain the area's position on the current weather-charts.

The Orbits of Jupiter's Satellites.

THE Cape Heliometer Observations with Mr. de Sitter's discussion* form the first material addition to first hand knowledge of the constants of Jupiter's system since the publication of the 'Tables Ecliptiques' of Damoiseau in 1836. In estimating the possibilities of the method and the actual gain of knowledge resulting from this work, it will be convenient to review the position in which the problem at present stands.

Damoiseau's tables, which have been used by all ephemerides, and carried on by different continuations since they terminated in 1880, are a very unsatisfactory piece of work, if judged by modern standards. Briefly, they represent a half-finished approximation with all the discrepancies of such a stage left standing. Damoiseau's explanatory matter is very curt, but to a careful reader there can be little doubt as to his procedure. His work is professedly a continuation and recalculation of the tables of Delambre which expired in 1840. Delambre's formulæ are figure for figure identical with those of the *Mécanique Céleste*, adding only the equation of light, and the reduction of the longitude from the plane of the satellites' orbit to that of Jupiter's orbit. Damoiseau has marked with an asterisk his own additions, and they consist of certain terms in latitude due to the substitution of the mean longitude for the true longitude as used by Laplace, of certain terms of long period of which the theory had been given by Laplace after the publication of Delambre's tables*, and of some other very trifling inequalities. We may safely assume that he added nothing else of importance either to theory or to method of discussion. We may then conclude that he rewrote Laplace's formulæ to some extent, evaluated the smaller terms with values of the masses, &c., deduced from Delambre's tables, that he cleared the observations of the effect of these terms and proceeded to find the elliptic elements, the coefficients of the great inequalities, and the other data, selected by Laplace for determining the masses and Jupiter's ellipticity. Before the days of least squares there was no method available for such a purpose except that of grouping the observations so that each unknown in succession came to the fore, and approximating repeatedly until a satisfactory set of solutions was obtained. Such a method is fairly suitable for a straightforward problem with large numbers of well-distributed observations, where all the periods to be discussed separate clearly from one another and are not too long. But in the problem of Jupiter's satellites we have the antithesis of each of these requisites. For instance, a case of very long period is that of the motion of the perijove of IV., which revolves in about 520 years; and a case of nearly equal periods, that of the two eccentricities affecting III., which separate by only 2° per annum. A still more formidable difficulty arises in the discussion of the instant of eclipse of the satellite, and in the form of Jupiter's shadow, and this has been treated by Laplace in far too summary a manner.

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* 'Discussion of Heliometer Observations of Jupiter's Satellites made by Sir David Gill, K.C.B., and W. H. Finlay, M.A.' By W. de Sitter. Groningen: J. B. Wolters, 1901.

It is no wonder that data so determined should be defective, and we may well admire the industry and acumen of Delambre, who first made with these tools and materials a determination of data which is substantially sound.

Damoiseau states that his two determinations of the node of Jupiter's equator upon his orbit differed by $2' \, 43''$ and of their inclination by $2' \, 47''$: beyond this we have hitherto had no direct means of judging the goodness or badness of his results; but it is well known that as far as they can be judged by internal comparisons they come out very badly. For example, Damoiseau appropriates Delambre's coefficient of the great inequality of I. from which the mass of II. is found ($3^{\mathrm{m}} \, 13^{\mathrm{s}}\cdot079$) without apparently anywhere determining it; and his minor inconsistencies are too many to be told; but the greatest stumbling-block has always been that he never completed his own approximation by discarding the masses borrowed from Delambre, determining those indicated by his own constants and recalculating from these the smaller inequalities.

The agreement of values as originally adopted with the results of such a recalculation would have given the only possible test of the adequacy of his own work; he has not made this test, although in one case he has altered Delambre's coefficient from

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* Laplace's calculations and consequently those of Damoiseau were shown to be at fault by Adams: *v.* Adams's 'Collected Papers,' ii. p. 224.

$116^{s}{\cdot}76$ to $65^{s}{\cdot}07$, and, as Adams showed [*], the changes which this implies halve the mass of IV. and nearly double that of I.

Theory and tables are mutually necessary in order that either may be improved by help of observations. M. Souillart of Lille, who devoted infinite pains to a revision of the theory of the satellites and added several very sensible terms of the second and higher orders to those given by Laplace, undertook to supply this theory to Damoiseau's tables. Damoiseau has himself left a memoir in MS., designed to elucidate the construction of his tables, and this was in M. Souillart's hands, but we learn little or nothing of the points of present interest—the manner in which Damoiseau selected and discussed his observations.

Souillart's work is an admirable and finished piece, and cannot fail to be of service to the future investigator as a parallel to his own researches; but it is of an academic character, with little or no trace of the observatory or computing-room upon it, and in the course of reducing his theory to numbers he has made what appears to me a suicidal error of judgment. "Tous les nombres," he says [†], "relatifs aux satellites, dont nous ferons usage ici, seront ceux que Damoiseau a donnés explicitement et ceux qu'on tire de ses formules. Outre l'avantage de présenter ainsi un système d'éléments associés, dépendants les uns des autres, ce choix aura encore celui d'en faciliter la correction ultérieure : puisque la comparaison a été faite déjà entre les Tables de Damoiseau et les diverses observations, il suffira de comparer nos formules à celles de Damoiseau." That is to say, besides giving body to his own formulæ by expressing them in numbers, Souillart proposes to render them useful as a consistent theory deducible from Damoiseau's constants. Damoiseau's inconsistencies leave a number of courses open, but as to the best of these there can be neither doubt nor difficulty.

By the methods of the *Mécanique Céleste*, livre viii. ch. ix., he should have obtained the values of the masses, &c., from the numbers given by Damoiseau—as Adams actually did. With these he should have calculated the coefficients of the minor inequalities which were not derived from observation. We should then have had a theory which was identical with Damoiseau's in all the most prominent features, and differed by trifling amounts in other respects. In place of doing this he has selected data from Damoiseau's *Introduction*, whether belonging to Damoiseau's own determination or to the numbers taken from Delambre, which allow him to present a faultlessly consistent scheme from beginning to end. This arbitrary procedure results in the obliteration of the only elements of interest in Damoiseau's work—his original determination of certain elements and coefficients. Thus it results in the alteration of the coefficients of the great inequalities of I. and II.—probably the best determined data of observation of the whole system—by 9″·6 and 41″·1 respectively. Its effect upon the eccentricities is even more anomalous.

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[*] Collected Papers, ii. p. 188.

[†] "Théorie analytique, seconde partie," Mém. Acad. Sciences, tom. xxx. no. 1, p. 3.

While the tables were still empirical it was discovered by Wargentin that the equation of the centre of III. could not be expressed by a single term, and Laplace analysed it into two main parts—one relative to the perijove of III. and the other relative to that of IV.; the coefficients of these Delambre put at 263″·7 and 116″·8; Damoiseau made them 258″·7 and 65″·1. Souillart treats them by uniting in a single term at his own epoch (1850) the effect of Damoiseau's two coefficients and redistributing their joint amount in accordance with the masses he has adopted, making them respectively 220″·3 and 115″·2. In fact, Souillart having adopted from Damoiseau the masses which the latter adopted from Delambre, forces back into a rough accordance with Delambre the material which Damoiseau provided for a new determination of these very masses. R. A. Sampson.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Efficiency of the Curved Plate for Charting Purposes.

Gentlemen,—

In the last number of the *Observatory* Prof. Turner, in reviewing that portion of my annual report which deals very briefly with the results obtained last year with curved-plate cameras, pleads "for a little more accuracy in....numerical statements." I did not think it necessary to go into the details of the arithmetical and geometrical computations by which the figures criticized were arrived at, but if Prof. Turner wishes it I will do my best to satisfy him. The area of a segment of a sphere is

$$2\pi r^2(1-\cos\theta/2),$$

where θ is the angular field covered by the plate. The area of the hemisphere is $2\pi r^2$, and of the entire sphere $4\pi r^2$. Hence the number of plates required to cover the entire *visible* sky (at the very most a hemisphere) and the entire sky are (allowing nothing for overlapping) respectively

$$\frac{1}{1-\cos\theta/2} \text{ and } \frac{2}{1-\cos\theta/2}.$$

For the three different instruments compared in my report, which have fields respectively of 45°, 5°, and 2° in diameter, these numbers (computed to the 4th place) would be:—

Instrument.	Visible sky (hemisphere).	Entire sphere.
Curved-plate doublet (45°)........	13·13	26·26
Bruce 24-in. doublet (5°)	1053	2116
Astrophotographic 33-cm. ob. (2°)..	6667	13334

To the nearest whole numbers, therefore, the comparative figures are 13, 1100, and 13,000, instead of 16, 1000, and 14,000 as given in my report. The more exact figures are, as a matter of fact, more favourable to the case of the curved-plate camera than those given in the report.

Prof. Turner's criticisms and the figures he gives seem to be based on two mistakes. The first one is evidently due to a hasty reading of the paragraph criticized, by which he omits to notice that the figures given for the first two instruments are those for the *visible* sky, that for the last for the *entire* sky. If the comparison made in this way appears in any way unjust or misleading, I can only say that I greatly regret it; there was not the slightest intention to convey such an impression. If I had said in the first case *entire sky visible at any one station on any one evening* instead of using the shorter expression *entire visible sky*, any possible chance of misinterpretation would have been avoided. I can hardly think, however, that the language, even as it stands, has been generally misunderstood. The second mistake which Prof. Turner makes is that of comparing the area covered by a circular field with that covered by a square field. In comparing different lenses the diameter of the maximum *circular* field of good definition is obviously the only proper criterion. Square plates are used, it is true, in the Astrophotographic Survey (I hope Prof. Turner won't object to this term also); but in the plates that I have seen the definition in the corners is bad, and the good field is, if anything, something *less* than 2° in diameter, and not 2° square. I have understood also that, for this reason, stars in the extreme corners of these plates (which appear near the centre of the second series) are not measured.

I dislike to reply to Prof. Turner's criticism of my very brief and therefore necessarily incomplete statement regarding the beginning and progress of the work of the International Survey. He attributes to me ignorance concerning this, one of the most important pieces of astronomical work that has ever been undertaken, and further implies, by his entirely unnecessary defence of the delay that has ensued in completing the work, that I have criticized or reflected unfavourably upon the progress that has been made. I have not been engaged personally in any of the work of the survey, but I have followed with considerable care and a great deal of interest the official reports of the various conferences that have been held in Paris. The call to the first conference was issued in October 1886 (not in 1887, as stated by Prof. Turner), although the conference did not actually meet until the following April.

For more than two years previous, however, Admiral Mouchez had been in correspondence with the directors of various observatories in regard to the details and scope of the proposed work, and a great deal of experimental work had been done by the Henry brothers, by Prof. Pickering, by Prof. Pritchard, by Dr. Gill, and by others. The first session of the conference met more to discuss results already secured, and perfect details for the further uniform and systematic continuation of work already begun, than to map out, *de novo*, a new scheme of work. Considering this, and the further fact that the conference decided to use the same type of instrument and the same method of work that had been in use by the brothers Henry at the Paris Observatory for two years, it would not perhaps be wrong to say, if we wanted to split hairs in such a discussion, that the work had really been begun in 1885, rather than in 1886, or 1887, or at any subsequent date.

There was not the slightest intention on my part to reflect in any way upon the very great value of the work that was mapped out at Paris, nor upon the progress of the work since it has been begun. For one, I can assure Prof. Turner that I do not think any defence or explanation on either of these points is at all necessary. Every one who has paid any attention at all to the subject now fully realizes the real magnitude and difficulties of the programme that was mapped out, and will commend rather than condemn the progress that has been made toward completing it. But I think I can add, without fear of being misunderstood, that I have always believed, and still believe, that a *mistake* was made in not adopting a type of instrument having a shorter focal length and a much larger field (the last in accordance with Prof. Pickering's suggestion in 1885), and that a further mistake has since been made in attempting to measure too large a number (*i. e.* too faint) of stars on each plate rather than rely on the plates themselves for future reference and comparison. The final charts and published measures will lose much of their value from their very bulk, to say nothing of the expense, which in the case of a good many observatories will be absolutely prohibitive.

The increase in the size of the field on each plate and the reduction of the scale would enormously decrease the cost of reproduction and the bulk of the whole chart, and prompt reproduction (on glass) would entirely obviate the difficulty in regard to the fading-out of the fainter images, of which we have heard a good deal lately. On my own plates I have never noticed any trouble of this kind, and I am inclined to think that it is only experienced on plates of very short exposure or on those that have been underdeveloped or improperly fixed and washed. I have made it a practice to develop my star-plates at least one hour, generally two, and to use unusual care in fixing and washing. But whether this difficulty is to be found at all in the case of the negative, it certainly is not to be in the case of the glass positive.

In conclusion, it may be said that the questions of reproducing and measuring the original curved-plate negatives, which Prof. Turner seems to regard as matters of considerable difficulty, are in reality quite simple, and offer far less difficulty than the problem of commercially making the sensitive plates themselves. The negatives can be easily reproduced on flat plates if it is desired without any loss of definition or distortion. But this is only necessary for pictorial effect or for the purpose of making paper prints by contact-printing. For measuring I have designed a machine (which is partly built), which measures the position of a star on the curved plates (in polar co-ordinates) with the same or even greater ease, rapidity, and accuracy than it can be measured on a flat plate in rectangular co-ordinates.

As I have stated in this year's report, all of this work will be published in full, with reproductions from negatives, &c., at the very earliest opportunity. It has been greatly hampered and delayed by lack of assistance, by illness, and by the large amount of work connected with the building and equipment of the new observatory.

Allegheny Observatory,
1901, May 20.

F. L. O. WADSWORTH.

[The above letter has been shown to me at Prof. Wadsworth's express request. I think he has taken my remarks rather seriously; but I am quite prepared to stand by them in the same spirit if he wishes it. In the first place, the number of plates required to cover the sky (either visible or entire) is *not* to be obtained by dividing the area of the sky by the area of a plate. You must fit the plates in so as to "cover the sky," and this means a certain amount of overlap. With circular plates it means a *very large* overlap. I took square plates because they were much more favourable to Professor Wadsworth's figures; but if he insists on circular plates he must increase his figures to nearly double: perhaps more than double—I have not just now the leisure to work out a scheme. However, my request for "a little more accuracy in numerical statement" seems to be fairly justified if three numbers quoted as 16, 1000, and 14,000 should be really 13, 1100, and 13,000 on Prof. Wadsworth's own showing. Secondly, as regards the date of commencement of the Chart work. Prof. Wadsworth's words (quoted in my note on p. 213) were: "the work begun in 1886 at 17 different observatories." He claims to justify this by saying that a *summons* to a Conference was issued in October 1886; but I entirely disagree with him. The very pattern of instrument to be used was not settled till April 1887; and it was quite uncertain what observatories could take part in the work for some time after this. For instance, Oxford: it was an unexpected piece of generosity on Dr. De la Rue's part which allowed the University Observatory to take a share in the work; the late Prof. Pritchard thought that his chance of taking part was gone when the reflector was not chosen as the standard instrument. The whole thing was quite in the air: and how "17 different observatories" can be said to have "begun work" by being summoned to discuss whether they (or others) shall do it all, and if so with what instruments, and if so how and when these instruments are to be got, passes my comprehension. I should have thought it much simpler to accept a friendly correction of one or two figures and dates than to attempt to defend them in this very strained manner.—H. H. T.]

William and Samuel Molyneux.

GENTLEMEN,—

The great assistance which Samuel Molyneux gave to Bradley by providing the means and taking part in the observations at Kew which ultimately led to the discovery by the latter of aberration, though Molyneux did not live to know of the great result, has rendered his name familiar to astronomers. His acquaintance with Bradley commenced in 1723; two years afterwards the zenith-sector by Graham was mounted in the house at Kew which his marriage with Lady Elizabeth Capel, in 1717, had brought into his possession, and Bradley took part in his observations there until 1727, in which year Molyneux became a Lord of the Admiralty and Bradley obtained an instrument of his own, which he set up in the house of his aunt, Mrs: Pound (whose husband had died in 1724), at Wanstead. Next year, 1728, Molyneux died, and in the following, 1729, Bradley announced his first great discovery, that of aberration.

But, though the name of Samuel Molyneux is well known, that of his father, William Molyneux, is less so than it should be. Born at Dublin in 1656, he first came to England in 1675, and studied law in London. He returned to Dublin in 1678, and married in the same year. A second visit to England had for its object the obtaining medical advice for his wife, and a longer sojourn commenced in 1689, the cause of which was the war which followed the political troubles in Ireland after the revolution. He settled at Chester, where his only son, Samuel, was born in the same year, and his principal work, 'Dioptrica Nova,' was written and afterwards published in 1692. The copy of the first edition in the British Museum Library evidently once belonged to the author, and has several manuscript notes by him. In the fly-leaf is a list of those to whom copies were to be presented, one of whom was Flamsteed. After the seventeenth and eighteenth propositions, page [77] commences thus:—

"*Another solution of the Two last Problems in Prop.* xvii. & xviii.

"These two last Problems were likewise communicated to me, most ingeniously solved by my fore-mentioned Honoured Friend Mr. J. FLAMSTEED. The Solution I shall give in his own Words at large, as I have them in a Letter to me Dated from the *Greenwich Observatory, Jan.* 17, 168$\frac{4}{5}$, as follows."

Flamsteed's solutions follow, but Molyneux little thought what the consequence would be of placing them *after*, instead of *before*, his own. This we learn from his Autobiography, written to his brother in 1694, and published by Sir Capel Molyneux as part of 'An Account of the Family and Descendants of Sir Thomas Molyneux' in 1820. William Molyneux there writes, at p. 76, after mentioning with a pardonable pride the number of scientific men with whom he had been on terms of intimacy or in correspondence:—

"Mr. John Flamsteed, the King's Professor of Astronomy at Greenwich, was formerly my constant correspondent for many years, many letters having passed between us; but upon publication of my Dioptricks, he took such offence at my placing a solution of his of the 16th, 17th, 18th propositions thereof, after, and not before, the solutions which I myself gave of the said propositions, that he broke his friendship with me, and that too with so much inveteracy, that I could never bring him to a reconciliation, though I have often endeavoured it; so that at last I slighted the friendship of a man of so much ill-nature and irreligion, how ingenious and learned soever."

Brewster quotes this passage in the second edition of his 'Life of Newton' (vol. ii. p. 162), and it certainly throws a side-light on the ever-to-be-regretted disputes between the latter and Flamsteed. Molyneux fully acknowledges the assistance he had derived from Flamsteed, and to me, I must confess, it seems quite incredible that the above was the real, and not merely the ostensible, cause of the offence taken at the book. It is much more probable that the true cause is to be found in the high terms used in speaking of Halley, as well as Newton, for commendation of the former was to Flamsteed like a red rag to a bull. The first of his letters to Newton in which angry expressions are vented is dated February 24, 1691-2 (Baily's 'Flamsteed,' p. 133), and these are in abuse of Halley. Whether Newton answered it or not is not known *, probably the wisest course was not to do so. This subject is a very unpleasant one, but as it had been started in an *ex parte* way by Baily, Brewster was quite right to point out that some things that Flamsteed wrote were misrepresentations, and also that several of his so-called letters were not really such, but drafts, written and never sent.

Although Molyneux was not much of a mathematician, he was able to appreciate the value of Newton's work, and mentions particularly (p. [273]) the striking confirmation of the truth of the law of gravitation afforded by the fact that Kepler's third law obtains in the movements of the satellites of Jupiter and Saturn round their primaries, as well as in those of the planets round the Sun; this, he says, "is noted by the admirably learned Mr. Newton, in his incomparable Treatise, *Philosophiæ Naturalis Principia*

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* His next letter to Flamsteed which has been published is dated October 7, 1694, more than 2½ years after the above.

Mathematica." Flamsteed wrote on October 31, 1713:—"I think his new *Principia* [*i. e.* the second edition thereof] worse than the old, save in the Moon." Yet in the second letter to Sharp, which I quoted last month, he says (under date 1710, September 20):—"On the whole, I plainly perceive there is a cause that vitiates their motions; and it seems to be their mutual gravitation on each other, and pretty regular. So that I do not doubt but to solve them: for their gravitations being reciprocally as the squares of their intermutual distances, and directly as the bulk of their bodies, it will not be difficult to determine how much they draw the remoter planet inward toward the Sun, or force the nearer from him."

Whether he had a clear idea of the difference between bulk and mass does not appear; but he certainly seems at last to have recognized that gravity acts according to the inverse square of the distance, though he carefully avoids referring to the fact that Newton had proved it, saying only that the latter had "prosecuted....Kepler's doctrine of magnetical fibres improved by Sir Christopher Wren....and I think I can lay some claim to a part of it."

William Molyneux returned to Dublin in 1690, and died there eight years later, soon after having made another journey to England, in order to pay a visit to Locke.

Yours faithfully,

Blackheath, 1901, June 7. W. T. Lynn.

P.S.—Perhaps it may be worth while to mention that the so-called Second Edition of the 'Dioptrica Nova,' which appeared in 1709, is only a reprint of the first, with altered titlepage.

OBSERVATORIES.

Royal Observatory, Greenwich.—The Astronomer Royal's Report for the year ending May 10 was read on Saturday, June 1, to the Board of Visitors, whose numbers were almost complete. The Report is in the usual form, but opens with a reference to the wooden fence which surrounds the New Observatory building. Provision has been made by the Admiralty for an ornamental iron railing to replace this, but the authorities in charge of the park have not as yet given permission for the addition of a small piece of land to the Observatory grounds, which is necessary to give this railing its best effect.

Transit-Circle.—The number of transits observed during the year was 10,938, of circle observations 9838. These are completely reduced to the end of 1900, and the reduction of the more recent observations is well advanced. The printing of the observations has been accelerated, and is about three months in advance as compared with last year.

The apparent correction for discordance between the nadir observations and stars observed by reflexion was found to be $-0''\cdot39$, which is not much different from the value that has obtained in the years 1892–1900. The observations of level and nadir continue to show the diurnal change noticed in recent years, as the level error at noon and midnight differs from that observed at 6 P.M. by $0''\cdot39$ and $0''\cdot23$ respectively.

The correction for the R—D discordance for 1900 was $+0''\cdot087+0''\cdot315 \sin$ z.d. The correction to the tabular obliquity of the ecliptic was found from observations in 1900 to be $+0''\cdot50$, and the mean error of the Moon's place was $-0^{s}\cdot128$ in R.A. and $-0''\cdot66$ in N.P.D., deduced from 115 observations. The Ten-Year Catalogue for 1890 has been published during the year, and as the special object of this was the re-observation of stars in Groombridge's Catalogue (1810), a new reduction of this catalogue from the original observations is in progress, to facilitate determination of proper motion.

The New Altazimuth.—This instrument is in good working order, and has been used during the year mainly as a reversible transit-circle in the meridian for observation of the Eros reference stars and fundamental stars. Observations have, however, been made in other azimuths with satisfactory results.

Equatorials.—18 disappearances and 10 reappearances of stars occulted by the Moon have been observed. The observations of Capella and other double stars have been continued with the 28-inch refractor. The 26-inch Thompson refractor has been used for taking photographs of Eros and also some of comet *b* 1900.

The work of the Astrographic Chart and Catalogue is proceeding. The plates are practically all taken, but some fields are being photographed again to replace plates which show signs of deterioration. The catalogue is being printed, and the measured rectangular coordinates for the zones 64° to 68° are in type. A good series of photographs for determination of the parallax of Eros, and a series of the new star in Perseus, have been obtained with the Astrographic equatorial.

The photographic spectroscope which is used with the 30-inch reflector mounted on the Thompson equatorial could not be used when the observation of Eros was on hand, but since March 28 very satisfactory photographs of the spectra of some of the larger stars have been obtained.

The section devoted to earth-magnetism contains some interesting reading. The question of the possible effect of disturbance from electric railways has required careful consideration. It appears that an electrical tramway with "well-insulated" returns is likely to have little effect on the magnets. The London County Council propose to adopt this system for their tramways near the Observatory, and it is hoped that the Board of Trade will lay down regulations to secure adequate protection for the magnetic work at Greenwich which has now been carried on continuously on the same general system for a period of 60 years and which could not be transferred to another site.

The Report contains a short account of the results of the observation of the Solar Eclipse of 1900, May 28, obtained at Ovar, and of the preparations made for the eclipse of 1901, May 18. It is stated that arrangements have been made for a re-determination of the Greenwich—Paris longitude in conjunction with observers from the Paris Observatory.

From the record of solar photographs it appears that the sun-spot minimum may be considered as reached, as no Greenwich photograph showed a spot since March 7.

BIDSTON OBSERVATORY, LIVERPOOL.—The work of the Observatory under Mr. W. E. Plummer's control is essentially of a utilitarian nature, so that his Report for the year 1900 contains little astronomy beyond the record of the observations with the transit instrument made to determine the clock-error. Right Ascension of certain circumpolar stars has also been determined with the same instrument, and some observations of comets and the diameter of Venus were made with the equatorial. But Mr. Plummer evidently intends to make a special study of earth-tremors, for he has now three seismometers of different patterns in the observatory. The meteorological results are given, as in former years.

TEMPLE OBSERVATORY, RUGBY.—The Report to the Headmaster for 1900, presumably by Mr. Seabroke, states that the work of the Observatory has been continued in a similar manner to former years, but owing to the bad weather not so much has been accomplished. The building was used on 44 nights, and 120 members of the School availed themselves of its opportunities.

The position and distance of 69 stars were measured, and the similar measures for the past five years are being prepared for publication.

PUBLICATIONS.

PUBLICATIONS OF THE U.S. NAVAL OBSERVATORY, SECOND SERIES, VOL. I.: SUN, MOON, PLANETS, AND MISCELLANEOUS STARS, 1894–99.—In accordance with the decision arrived at after the retirement of Professor Harkness, these observations no longer appear in annual volumes, and this is the first instalment on the new system. It contains all the transit-circle observations for the six years (with the exception of those for the 'Astronomische Gesellschaft Catalogue' Zone −13° 50′ to −18° 10′), amounting to nearly 14,000 observations, of which rather more than 3000 represent the net result after deducting the observations of clock and adjustment stars. Until June 1899 the observations were taken with the 9-inch transit-circle; afterwards with the new 6-inch steel instrument. The total includes about 600 observations of the Sun, and nearly as many of Venus, about 500 of the Moon, and 400 of Mercury, an average of over 100 for Mars, Jupiter, Saturn, Uranus, and Neptune, nearly 100 observations of some dozen of the minor planets, and rather more than four each of some 60 stars, all the observations being strictly differential and not fundamental.

We notice that with about the same aperture as the Greenwich Transit-circle, and a much more favourable latitude for observations of planets, Mercury is observed about three times as often as at Greenwich, and it would be interesting to know whether there was any *a priori* reason for the very rare observation of Mercury's limb (less than 25 per cent. in R.A. and only 1 in N.P.D.). The limbs of Neptune do not appear to have been observed at all, and those of Uranus only five times in R.A., and never in N.P.D. There seem to be also more observations of the centre of Venus than might be expected, though the magnifying-power in use is 25 per cent. less than that at Greenwich.

There is a detailed account of all the changes in the mounting and adjustment of the instruments in their new quarters, and the usual tables of instrumental constants. W. W. B.

PUBLICATIONS OF THE ASTRONOMICAL LABORATORY AT GRONINGEN.—Prof. Kapteyn, in No. 8 of these Publications, gives a preliminary discussion on the mean parallax of stars of determined proper motion and magnitude. The aim of the paper is to confute the idea that the mean parallax of fixed stars is nearly proportional to their proper motion. The parallaxes used are

(1) Those determined at the Cape;
(2) The determinations of Elkin at Yale;
(3) The results of investigations with the Leipzig heliometer by Peter;
(4) Prof. Kapteyn's own results;
(5) Four from various sources;
(6) The parallaxes determined by Flint.

The first results include all the stars of whatever type, and give:—

Magnitude (m).	P.M. (μ).	Parallax (π).
6·15	4″·33	0″·257
5·31	1 ·43	0 ·135
4·46	0 ·50	0 ·164

Finally formulæ are derived connecting the three quantities (I) for stars of Type I., (II) those of Type II., (III) for all the stars together:—

$$\text{(I)}\quad \pi_{\mu.m} = (0{\cdot}905)^{m-5{\cdot}5} \times \sqrt[1{\cdot}11]{0{\cdot}116\,\mu}.$$

$$\text{(II)}\quad \pi_{\mu.m} = (0{\cdot}905)^{m-5{\cdot}5} \times \sqrt[1{\cdot}54]{0{\cdot}0262\,\mu}.$$

$$\text{(III)}\quad \pi_{\mu.m} = (0{\cdot}905)^{m-5{\cdot}5} \times \sqrt[1{\cdot}505]{0{\cdot}0387\,\mu}.$$

$\pi_{\mu.m}$ being the parallax in seconds of arc of a star-magnitude m, whose annual proper motion is μ''.

A table of 58 measured parallaxes is appended.

SELENOGRAPHIC TABLES *.—This work contains an exhaustive discussion and tabulation of all the formulæ required for the reduction of physical observations of lunar objects and measurements of their position. The formulæ themselves are not new, but it is very convenient to have them collected. The tables for computing the position of the terminator at any time are given from 1790 till 1940. It may be mentioned that Mr. Goodacre has recently, in the *B. A. A. Journal*, extended them to 1999. These tables treat the geocentric and selenocentric longitude of the Sun as identical. The resulting error in the longitude of the terminator may amount to 9′. The lunar ephemeris in the *Monthly Notices* corrects for this point, and is therefore somewhat more accurate than these tables; but the difference is scarcely appreciable. It should, however, be considered in the careful reduction of large-scale photographs.

NOTES.

COMET NOTES.—Descriptions of the Great Comet are pouring in, and some beautiful photographs of it have been sent from the Cape. It is stated to have been fan-shaped, like that of 1861, with two straight and narrow principal tails, and a remarkable appendage to the south of these, large and broad, but less bright than the others. Major Eddie says in *B. A. A. Journal*, No. 8:—"On May 6 . . . . the bright straight tail with its faint extension was about 16° long, reaching nearly to Rigel . . . . .; while the enormous lateral emission of shimmering light flooded the S.W. sky with a ghostly gleam 40° long, 6° to 10° broad: while the whole space between this mighty extension and the more brilliant tail was filled with a gauze-like sheen." The existence of this singular appendage is abundantly confirmed by independent descriptions.

A. C. D. C.

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* Veröffentlichungen des königlichen astronomischen Rechen-Instituts zu Berlin.—No. 14. Formeln und Hülfstafeln zur Reduction von Mondbeobachtungen und Mondphotographieen. Für selenographische Zwecke, von Dr. K. Graff. Berlin, 1901: Ferd. Dümmlers.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—

Provisional designation.	Date of discovery.	Discoverer.	Magnitude.
GK	April 24	Wolf.	14
GL	May 21	Wolf, Carnera.	11·5
GM	May 23	Wolf.	11
GN	May 18	Carnera.	11

GK was not observed again, and must therefore be considered as lost. An announcement of a planet, GO, was made on June 9, but this has been withdrawn as erroneous. A. C. D. C.

OBITUARY.—DR. ADOLPHE HIRSCH [*]. We record with regret the death of Dr. Hirsch, the Director of the Neuchâtel Observatory, which took place in his 71st year, on 1901 April 16.

Adolphe Hirsch was born at Halberstadt, in Germany, and received his education in that country. He studied under Encke at Berlin, and was afterwards Assistant to Leverrier at Paris. When the Government of the Canton of Neuchâtel proposed to establish an observatory, Hirsch was consulted as to its construction, in 1858 was appointed its Director, and entered upon his duties in the spring of 1859. The chronometer department, which has naturally been of so much value to the industry of the Canton, was established by him in 1860.

Hirsch devoted himself specially to geodesy. In 1861 General Baeyer proposed an association of the countries of Europe to further geodetic enterprise, and one result of this was the establishment of the Swiss Geodetic Commission, of which Hirsch was an active member and secretary for thirty-one years. He represented Switzerland on the Permanent European Commission, and was nominated Perpetual Secretary when this Commission changed its title to International Geodetic Association in 1886. Dr. Hirsch resigned this position in 1900. He was one of the founders of the International Bureau of Weights and Measures, and his advice and help in connection therewith were highly appreciated. With his colleague Plantamour he directed most of the geodetic work in Switzerland, and took part in the level determinations made in that country.

Dr. Hirsch has testified to his devotion to science by leaving his fortune to the Neuchâtel Observatory.

THE SOLAR ECLIPSE OF MAY 18.—The latest information that we have received about the recent eclipse is contained in letters from Mr. Dyson and Mr. Maunder to the Astronomer Royal, which we are kindly permitted to publish. The first is from Mr. Dyson at Aver Gadang, and is dated May 21 :—

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[*] Extracted from a notice by Dr. R. Gautier in *Ast. Nach.* 3710.

I can't give much of a report yet as I have only had two nights' developing. However, I have got 6 Thompson's (all but the two Burckhalter's) done, 2 Dallmeyer, and 2 Unars and 4 spectra.

I think the Thompson's are fairly good, but, as the cloud was much denser than at Ovar, the polar streamers hardly show. The Unar shows clouds and stars. I have not identified the latter. The Abney gives a very pretty photograph. With the spectroscopes the corona given by Quartz is good—the Flint is not developed. The second flash was not obtained. I have not developed the first flash photograph yet, but shall be agreeably surprised if I get much.

Photography here is very difficult. The lowest temperature I have seen in the developing-room is 78°. That was at 3 A.M. At 9 P.M. it was 86°. However, I am struggling along with ice and alum. I am very much afraid some alum will be left on the plates. There is not too much water. It comes from the ship. A warrant officer has been of the greatest assistance, filling the bottles with water, putting things in the ice-chest, &c. &c.

The second letter from Mr. Dyson is dated from Padang, May 26 :—

I finished developing on Friday night, and we left for Padang soon after mid-day yesterday (Saturday). . . . we say good-bye to Sumatra at 5 o'clock in the morning of the 28th.

The Thompson photographs are not as good as I should have liked. They are fair with the exception of the Burckhalter's, which are complete failures—practically nothing shown. The others will give useful results. Some of the Dallmeyer and Unar photographs are good. They all show the clouds. The last Unar photograph taken—exposure 5 seconds—is a great deal better than the others and shows a great number of stars. It should at any rate settle the intra-Mercurial planet question. With the spectroscopes I have got very fair corona spectra: the first flash is good enough to fill up a few places where the Ovar photographs were not good; the second flash I did'nt get. Newall telegraphs that he has not got the rotation of the corona, but has obtained marked polariscopic results and good 1½-inch-scale pictures.

I believe Perrine at Padang did fairly well, also the Dutch at Painan: Barnard and the rest at Solok got nothing, but the Fort de Kock people (Humphreys, flash) had excellent weather.

At Aver Gadang it was rather annoying that 10 minutes after the eclipse the weather was perfect. A very few minutes would have made a lot of difference, and the good photograph with the Unar was due to Capt. Oldham getting late with his exposures.

Mr. Maunder's letter, written from Quatre Bornes, Mauritius, on May 26, brings unpleasant news. It seems that he caught fever at Pamplemousses on the Monday after the eclipse, but, thinking lightly of the matter at first, arranged to stay at the Observatory until the end of the week; having become worse on Wednesday, he was compelled to leave the lowland and go to the hills. The letter says :—

The fever, though not very severe, has been especially obstinate and has left me in a state of extreme weakness. The doctor has absolutely forbidden my sleeping at the Observatory again.

The photographs obtained were numerous and, as far as I can judge, very fairly satisfactory. Unhappily the Sun's limb was boiling very badly during the eclipse.

Photographic Photometry.—We are not aware that anyone has up to the present adapted a wedge-photometer to the measurement of the magnitudes of stars on a photographic plate, but it appears from a note in a recent number of the *Comptes Rendus* (vol. cxxxii. no. 18) that M. Baillaud, Director of the Toulouse Observatory, has begun a research on this principle. If a stellar plate is lighted up so that the stars appear black on a bright background, the black image can be made to disappear by the interposition of a certain thickness of the wedge, just as in the case of a bright star on a dark background, and because of the difference of degree of blackness the wedge-reading will be different for stars of different magnitudes; but naturally the method is complicated because of the difference of size of the star-images. M. Baillaud has begun to make observations, and in the above-mentioned note gives the preliminary steps in the formulæ that he hopes to evolve. We look forward to reading more on this subject.

The Number of Stars Photographed with Different Exposures.—M. Stratonoff, of the Tachkent Observatory, who is apparently making an exhaustive study on the important subject of density of stars in different parts of the sky (see *Observatory* for March, p. 132), published an interesting note in *Ast. Nach.* no. 3710. In the year 1899 he exposed a plate to the region *h* and χ Persei for 30 hours, which he believes is the longest exposure on record. On counting the stars on the plate he found that the number did not come up to expectation, and infers that this region is probably less rich than others.

The following figures, giving the number of stars per square degree in various parts of the sky with different exposures, are of interest:—

Exposure.	Number of stars per square degree.
Pleiades.	
h	
0·1	100
0·3	160
3·0	600
10·0	1,300
25·0	1,700
Ring Nebula in Lyra.	
0·5	600
1·0	1,900
10·0	6,000
20·0	10,000

Exposure.	Number of stars per square degree.
Near *h* and χ Persei.	
h	
0·1	250
0·4	600
2·0	2,000
5·0	4,500
30·0	11,000
Near η Argûs.	
3·75	10,000
12·0	50,000
24·0	100,000

The Astronomer Royal's Report contained some statistics of a similar nature. In the work of the Astrographic Chart and Catalogue exposures are given to each field of stars for lengths 20 seconds, 3 minutes, 6 minutes, and 40 minutes. The number of stars shown with these different exposures have been counted for the whole of the zone from declination +65° to +68°, with the result given herewith. It is necessary to explain that each field is shown on two plates which overlap, and naturally, owing to differences in photography, some stars are shown on a plate which are not on the corresponding plate covering the same field. Below are given the numbers of stars which are seen on two plates. The above-mentioned Report gives also the numbers of stars seen on one plate at least, which are somewhat larger than these. The second column gives the number per square degree in this zone.

	65° to 68°.	Per sq. deg.
Number in B.D. of mag. 9·0 and brighter	2,018	4·7
Total number in B.D.	5,326	12·4
Number shown on Astrographic plates with 20^s exp.	3,968	9·2
" " " " 6^m	21,558	50·1
" " " " 40^m	115,528	268·3

Double Stars.—Mr. Espin has published in *Ast. Nach.* 3717 a list and measures of 39 new double stars, and some measures of 84 wide pairs made with the 17½-inch reflector. The new pairs are rather wide, but in general consist of stars with a difference of three magnitudes. The measures are of Herschel pairs which have been neglected, Struve and Otto-Struve pairs.

The Parallax of μ Cassiopeiæ.—Yet another parallax of this star. Bessel used θ Cassiopeiæ as comparison star, and his measures gave −0″·12 as the parallax. When Dr. Davis reduced these measures, allowing for the proper motion of both μ and θ, he obtained the result of +0″·02. But Prof. Jacoby found the parallax of θ itself to be +0″·23, and allowing for this we get a value of +0″·25, from Bessel's observations, for the parallax of μ Cassiopeiæ. The full values, therefore, are:—

	″
Bessel	−0·12
" (reduced by Davis)	+0·02
" (allowing for parallax of θ)	+0·25
Otto Struve (distance measures)	+0·251
" (position-angles)	+0·425
Schweizer (micrometer)	+0·084
Pritchard (Oxford photos)	+0·051
" "	+0·021
Jacoby (distances, photos)	+0·275
Peter (distances, helio)	+0·12
Bauer (angles, photos)	+0·238

This recent determination by Mr. G. Bauer is in No. 18 of 'Contributions from the Observatory of Columbia University, New York,' and is deduced from eleven neighbouring stars found on 28 Rutherfurd plates.

OVER-EXPOSED NEGATIVES.—We have received a reprint of two papers from the 'Transactions of the Academy of Science, St. Louis,' by Prof. T. Nipher. The first paper treats of the methods of obtaining prints from over-exposed negatives. Eighteen illustrations are given, and it would seem that by proper treatment a sharp and detailed print can be obtained from a negative if exposed 11½ million times the ordinary exposure. The over-exposed plates may be developed either as negatives or positives.

The paper may be read with advantage by those having to deal with eclipse photographs. The other paper is on "The Specific Heat of Gaseous Nebulæ in Gravitational Contraction."

VELOCITY OF GROOMBRIDGE 1830.—It is well known that until a few years ago this star, known as the "runaway," had the largest known proper motion of any in the heavens. Adopting Prof. Newcomb's value of its most probable parallax, 0″·14, its velocity at right angles to the line of sight would be 150 miles per second. Prof. W. W. Campbell has recently determined, by means of spectrum photographs, its velocity in the line of sight. The results of four plates are in substantial agreement, and the mean of the two best gives a velocity of 59 miles per second, approaching. Prof. Campbell, in his note on this in *Pub. Ast. Soc. Pac.* vol. xiii. no. 78, adds: "the greatest interest of the observations lies in the fact that fairly accurate determinations of stellar velocities are shown to be possible down to the eighth or ninth photographic magnitude, provided their spectra contain well-defined lines."

THE COMET.—Besides the reports given in our Comet Notes we have also seen a letter from the surgeon of a ship which was crossing the Indian Ocean in the middle of May, in which he says that on several nights soon after sunset they saw a fine comet low down near the western horizon. The comet was first seen on May 10, and was lost sight of after May 15, when the ship coming northward was in latitude 21°·7 N. It appears from the careful drawings which accompany the letter that the object during this time was in Orion, with its tail about 3° long, lying parallel to the belt, and its head *preceding*. The motion of the comet was in direction about at right angles to the tail. Since writing the above we have seen the photographs from the Cape, which confirm this exactly.

THE SUN-SPOT.—The spot on the Sun, whose first appearance on May 19 was noted last month, is a long-lived one, for it came into view again at the next rotation only a little diminished in size, and had not disappeared when it reached the western limb. The magnetic records at Greenwich show no disturbances during the whole time the spot has been on the Sun.

MEMORIAL TO THE LATE MR. G. J. SYMONS.—A meeting of the Subscribers to the Symons Memorial Fund was held on Tuesday, June 11, in the rooms of the Royal Meteorological Society, 70 Victoria Street, Westminster. The Treasurer, Dr. C. Theodore Williams, presided.

The Executive Committee reported that the proposal that the Memorial to the late Mr. G. J. Symons, F.R.S., the distinguished meteorologist and founder of the British Rainfall Organization, should take the form of a Gold Medal had been approved, and that the sum of £713 14*s*. 7*d*. had been subscribed for that purpose. It was very gratifying to learn how greatly Mr. Symons was esteemed and his work appreciated, for the subscribers numbered 323 and the contributions ranged from 25 guineas down to half-a-crown. After paying for the dies for the Medal and the expenses of printing and postage, there remained a balance of £621 14*s*. 4*d*., which the Treasurer was instructed to hand over to the Royal Meteorological Society, for the interest on the same to be used for the awards of the Medal. It was resolved that the Medal should be awarded biennially for distinguished work done in connection with meteorological science, irrespective of sex or nationality.

The following gentlemen took part in the proceedings of the Meeting:—Mr. W. H. Dines (President, Royal Meteorological Society), Mr. C. Hawksley (President-elect, Institution of Civil Engineers), Sir Erasmus Ommanney, Dr. A. Buchan, Mr. W. Marriott, Mr. E. M. Eaton, Mr. R. Bentley, Dr. H. R. Mill, Mr. J. Hopkinson, and Mr. R. Inwards, all of whom spoke in appreciation of the work and character of Mr. Symons.

FLAMSTEED'S WIDOW AND DELAMBRE.—With regard to the P.S. to Mr. Lynn's letter in our last number, dated 1901 May 22 (p. 240), he desires us to state that he has since had an opportunity of consulting Chaufepié's 'Nouveau Dictionnaire Historique et Critique' (presumably the one referred to; the name of Flamsteed does not occur in Bayle's Dictionary, to which this is a supplement), and that it says of Flamsteed (vol. ii. F, p. 53 *): "Il était marié, mais il n'a jamais eu d'enfans." The mistake is therefore Delambre's own, and it will be remembered that this portion of his work was published after his death, the editor (Mathieu) remarking that the author's health was failing much at the time when he was putting his last hand to it.

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* Each letter in this work is separately paged.

PERSONAL EQUATION.—Everyone interested in this subject should obtain the small but valuable pamphlet 'Sur la Détermination des Positions absolues en Astronomie' by M. Paul Stroobant, which is the outcome of an endeavour to show what has been done at various observatories to determine the "Equation." There are illustrations of the machines already constructed, with short descriptions of the principles and results, and M. Stroobant is to be congratulated on the happy thought which led to the collection of these. The pamphlet forms part of the 'Annuaire' of the Brussels Observatory for 1902, but can be obtained from M. Hayez, 112 Rue de Louvaine, Brussels.

THE TIME OF NEW ZEALAND.—Sir James Hector, K.C.M.G., Director of the New Zealand Institute and of the Observatory, has written to us to point out a mistake in an article on Universal Time in our February number of this year. On p. 90 it is stated that the time of New Zealand is 11^h fast on Greenwich, whereas it should be $11\frac{1}{2}^h$ fast. This time was adopted by the New Zealand Government as far back as the year 1868, when the present Time-Ball Observatory was established, so that New Zealand may be considered as possibly the first country to take up the zone-time system, if we may consider zones differing from Greenwich by an odd number of half-hours to properly come within that scheme. Sir James Hector kindly sends a copy of vol. i. of the 'Transactions of the New Zealand Institute' (1868), which contains the report of the meeting at which he recommended this time; and he also tells us that in 1860 he pointed out the modification of time-reckoning that would be necessary on the long route of the Canadian Pacific Railway.

THE WEATHER IN JUNE.—The drought which prevailed during the greater part of May, continued as a partial drought throughout June, the rainfall from May 30 to June 29 amounting to 0·83 inch on 10 days. The heavy rain on June 30 produced an additional fall of 0·90 inch. The month of June, although for the most part bright and sunny, was cold. The recorded sunshine generally amounted to 232 hours, which is equivalent to 47 per cent. of the possible duration, but there were only 12 days on which the temperature exceeded 70°. The highest temperature in the month was 79°·7 on the 9th, and the lowest 42°·7 on the 13th. On the 13th, which was a very cold day, the thermometer never rose above 58°·3; and on the 21st, one of the few warm days in the month, the minimum temperature was not lower than 58°·7. The lowest mean daily temperature, 51°·0 on the 13th, was 7°·8 below the average; the highest mean daily value, 66°·4 on the 21st, was 6°·1 above the average; and the monthly mean was about half a degree in defect.

Retirement of Sir Norman Lockyer.—it has been stated in several places that Sir Norman Lockyer will relinquish his duties as Professor of Astronomical Physics at the Royal College of Science, South Kensington, at the end of this year, but that he will retain his position as Director of the Observatory.

Sir Norman's connection with the Department of Science and Art dates from 1875. Before that he was attached to the War Office as editor of "Army Regulations."

Erratum.—June number, p. 225 line 17, *for* May 15 *read* May 1; line 19 *for* after that *read* after discovery.

Mr. Arthur Mee, in sending us a copy of his little Journal *The Cambrian Natural Observer* for May, calls attention to a short article on "The Grave of Prebendary Webb." It seems that the tombstone in Mitchel Troy churchyard is in a bad state of preservation, and the inscription makes no reference to the fact that Mr. Webb was a well-known astronomer. Mr. Mee suggests that astronomers should raise a worthier memorial.

Mr. Mee's Magazine is an interesting "permanent record of natural phenomena," and the Editor evidently aims at conciseness and humour. Records of lectures, meetings, and observations are given in short paragraphs of about a dozen lines, among which we find the following:

> A rapid variability is reported in the light of Eros, and it has been suggested that the object is double. Perhaps it was attacked by a canal too large for itself.
>
> In the account of a Sunday School celebration it was written that "two obliging cornets headed the procession." The printers changed this into "two obliging comets."

The Astronomer Royal was "At Home" to large and distinguished companies of ladies and gentlemen on June 18 and 21. The weather was splendid on both days, so that the guests were able to roam round the Observatory grounds and buildings with due enjoyment. It is scarcely possible to give a representative list of those who were present, but Surgery was well represented by Sir Henry Thompson and Sir Wm. McCormac, Travel by Mr. H. M. Stanley, the Navy by Admirals Keppel and Molyneux, and Science generally by many well-known men.

It was rather startling to see a newspaper paragraph headed "The Lick 36-inch Telescope shattered"; but on reading the details it seems that it referred to a mirror of 36-inch diameter, which is being made in a Pittsburg workshop for a projected expedition from the Lick Observatory to the southern hemisphere, and which was broken in course of manufacture. The expedition will necessarily be delayed.

The re-determination of the Paris—Greenwich longitude, which, according to the Astronomer Royal's Report, would be begun next October, will probably be postponed until the spring of 1902.

We offer our congratulations and best wishes to Mr. P. H. Cowell, Chief Assistant at the Royal Observatory, Greenwich, on his marriage to Miss Phyllis Chaplin, which took place on June 24.

FROM AN OXFORD NOTE-BOOK.

In connection with the International Catalogue which is now started, a question which presses for solution is that of the arrangement of stars on a uniform plan. There are several methods which suit special cases, and yet none of them seems wholly satisfactory in general. Some Catalogues have been arranged in alphabetical order of the Constellation-names, and this system has many advantages; but it will not suit an International Catalogue, unless we agree to adopt the Latin names; and it is not an essentially scientific plan. The order of R.A., combined with division into zones, as in Argelander's D.M., is perhaps the best practical working system. but it has the disadvantage of becoming obsolete as the epoch changes. We might retain the epoch 1900·0 for many years, as it bids fair to be of great importance; but it is sure to become ultimately unsatisfactory for meridian work, and the epoch 1875·0, which is apparently being adopted for the big German Catalogue already projected, will conflict with it. Is it too late in the day to go back to longitude and latitude for the stars? It seems a wild idea, but it may not be so impracticable as it seems. The absolute longitude changes, of course, but the *order* in longitude is not affected, and for purposes of a permanent catalogue this is very important. If we are going to observe planets with the heliometer or by photography, the places of the stars tabulated in latitude and longitude will be far more convenient than those tabulated in R.A. and Decl. Astrographic measures can be converted into latitude and longitude quite as readily as into R.A. and Decl. The only difficulty is the conversion of the existing catalogues of R.A. and Decl. into lat. and long.; but this initial labour, heavy though it might be, would ultimately be compensated by the saving in working up precessions on our present cumbrous system.

Taylor's Madras Catalogue, 1835·0, has been revised and republished by the Superintendent of the *Nautical Almanac.* It is a good big piece of work to have done — 11,000 stars — and Dr. Downing must be glad to see it complete. A comparison with Newcomb shows rather large discordances; *e. g.* those in R.A. are represented sensibly by

$$-0^{s}\cdot090+0^{s}\cdot025\sec\delta+0^{s}\cdot067\tan\delta.$$

The observations of collimation and azimuth both depend on observations of terrestrial marks; and it seems possible that by re-discussing the tabular azimuths of these marks, these systematic errors might have been eliminated. Dr. Downing has preferred, as he tells us in the Introduction, "not to apply a correction of an *a posteriori* character." But will not people who use the Catalogue apply this correction? And they might have been saved the trouble.

In the memoirs of the Great Comet of 1901 the following two letters from the *Cape Argus* are worthy of a prominent place. Prof. Barnard's photographs have shown us wonderful things happening to the tails of comets, but these naked-eye observations far surpass them in interest. Without rejecting the observations entirely, it seems possible that we have here something similar to the "jumping stars" (Sternschwankung), of which many observations have been made. The accordance of testimony on a particular date is remarkable, and the explanation given by one of the observers quite ingenious:—

The Comet.

To the Editor of the Cape Argus.

Sir,—On Sunday evening immediately before 7 o'clock, while observing the comet with a strong glass, I noticed the tail had a peculiar vibratory motion like the pendulum of a clock. An astronomical friend of mine explained the phenomenon in the following way:—While travelling through space the comet is necessarily affected by neighbouring planets, which exert a drawing or pulling influence in a certain direction. By reason of the tremendous pace at which the comet is travelling, it at a certain point quickly escapes the influence of one planet, immediately to come within the power of another, and this sets up the oscillatory motion which I observed. I think the argument is feasible, but I would like it confirmed by some of your readers.—I am, etc.,

Sea View, Sea Point. H. E. Slattery.

To the Editor of the Cape Argus.

Sir,—A really extraordinary thing happened last night about seven o'clock, and although some of my friends have pulled my leg about it, I am sure it was not an optical illusion, that I rush into print. While watching the comet intently I distinctly saw the tail move about four times up and down, the distance covered being about half its own length; the whole movement was slow, and, as far as I could judge, took about 12 seconds, after which it remained

> quite stationary. I should be glad if you would kindly insert these few lines in your valuable paper; it may happen that some of your readers are in a position to confirm this.—I am, etc., B. Trestare.
>
> St. Germain, Rondebosch, May 6.

With regard to this same comet, Mr. C. V. Boys has kindly sent me an extract from a letter received from Mr. F. B. Parkinson, A.R.S.M., Baviaan Krantz (24° 30′ E., 29° 47′ S.), in which he says, under date April 27:—

> I have noticed a pretty little comet for the last few days in the east before sunrise; it is sufficiently bright to see after the Sun has risen, and no doubt would be so all day if one knew the spot to look at; but no doubt you know more about it than I do, as I have seen no elements of it yet."

It seems quite possible that Mr. Parkinson may have been the first to notice the comet, though he does not give enough information to enable one to judge. Mr. Boys tells me he is a good observer, and in the same letter is given a striking account of the bursting of a large meteor.

The crossing of the Line by the Royal party the other day afforded an opportunity for our Sailor Prince to revive the old sailors' custom of receiving a visit from Neptune and his family, which was carried out with all proper ceremonies, not forgetting the "splicing of the main-brace." The opportunity was also seized (by the reporters) for referring to the occasion when the Equator was "spoken disrespectfully of." The story is generally quoted inaccurately, as most stories are, and it may be worth while to put it down here for reference:—

> The reigning bore at this time in Edinburgh was ——; his favourite subject, the North Pole. It mattered not how far south you began, you found yourself transported to the North Pole before you could take breath; no one escaped him. My father declared he should invent a slip-button. Jeffrey fled from him as from the plague when possible; but one day his arch-tormentor met him in a narrow lane and began instantly on the North Pole. Jeffrey, in despair and out of all patience, darted past him, exclaiming "D— the North Pole"! My father met him shortly after boiling with indignation at Jeffrey's contempt of the North Pole. "Oh, my dear fellow," said my father, "never mind; no one minds what Jeffrey says, you know he is a privileged person; he respects nothing, absolutely nothing. Why, you will scarcely believe it, but it is no more than a week ago that I heard him speak disrespectfully of the Equator."

The above is a faithful extract from the 'Memoirs of Sydney Smith,' by His Daughter (p. 20); and those who may want to use the story in connection with the forthcoming Antarctic Expedition or otherwise will be able to refresh their memories of the exact circumstances.

ANOTHER story which came the other day suggests a possible line of action for Jeffrey which might have afforded relief to his feelings without hurting those of other people—he might have spoken in a foreign language. A friend of mine has been cautiously sounded as to his willingness to "formulate the objections to the Nebular Hypothesis" in plain English. A timid opponent of the Hypothesis has ventured to speak disrespectfully of it *in Russian*, "because of the misapprehensions excited by his opposition to Laplace"; but a bolder assailant is desired. If Jeffrey had d—d the North Pole in Russian, he would merely have bewildered his arch-tormentor, not enraged him; but then we should have lost Sydney Smith's remark, and perhaps it is as well that plain English was used after all. And if the translation of the objections to Laplace's Hypothesis from Russian into English will give us a story calculated to stand wear and tear as well as that about the Equator, it is worth doing.

MR. H. G. WELLS has finished the first part of his clever story about "the First Men in the Moon" *. He has brought one of them back and destroyed his means of return; but he has left one of them in the Moon, and a second part of the story is promised dealing with the *signals* sent by this new kind of shipwrecked voyager. One of the pleasing features of the story is the extreme care obviously spent in thinking out the details of strange situations. Take, for instance, the following sentence describing the appearance of the Earth as an observer falls towards it from space:—

> At last the world seemed no longer a sphere, but flat, and then concave.

Now those who go up in balloons tell us of the concave appearance; and it is tolerably easy to imagine the spherical appearance when seen from a considerable distance. Somewhere between the two, therefore, it must seem flat; though this had never occurred to me until I read the above sentence. Mr. Wells does not labour the point—his reference to it is, indeed, so slight that it might easily be overlooked—but it has not escaped him; and the instance is representative of a good deal of thought of the same kind throughout the story. But I wish he would write *rather* more cheerfully: he always seems to be desirous of "making one's flesh creep." Perhaps we are going to have some cheerful news signalled from the Moon; it would be a welcome change.

* *Strand Magazine* for June.

o. 308.] AUGUST, 1901. [Price

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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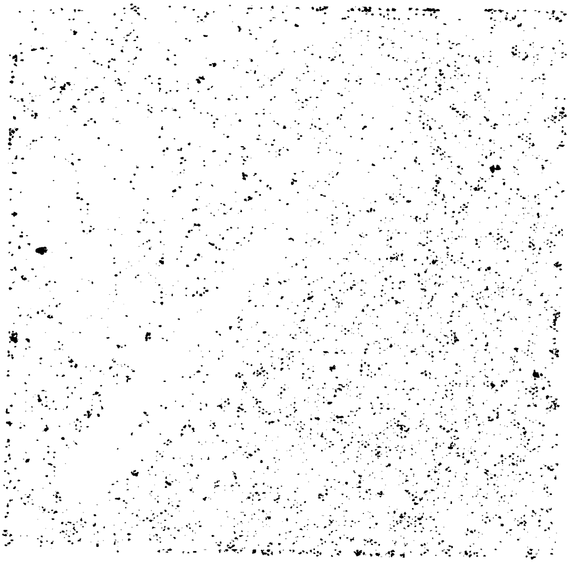
THE GREAT COMET OF 1901.



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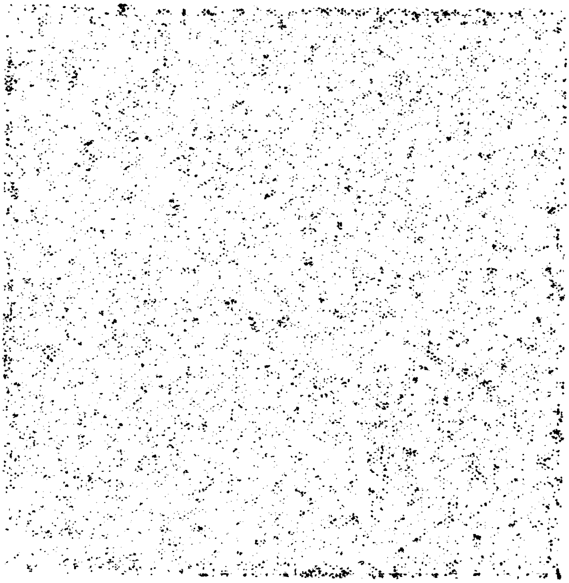

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIV. AUGUST, 1901. No. 308.

The Great Comet of 1901.

We reproduce, by kind permission of the Royal Astronomical Society, a photograph of the Great Southern Comet taken on May 4 with the McClean 24-inch telescope at the Royal Observatory, Cape of Good Hope, with an exposure of 15 minutes. The photograph gives an excellent representation of the principal tail, or tails, for there are two streams of unequal brightness proceeding from the nucleus, with a very decided dark space between them. Being taken in twilight, however, the exposure could not be carried far enough to show the longer but much fainter tail, at least it is not shown on the copies sent by Sir David Gill.

A second illustration is therefore given, taken from a drawing made by Mr. Lunt, of the Cape Observatory, which shows the dimensions and most striking features of the comet, from eye observations and photographs, as well as its position among the stars on May 12.

The long faint tail was first seen on May 3, when it was only just visible, but in a few days, as the comet receded from the Sun and was seen on a darker sky, the faint tail appeared four times as long as the main tail, and quite 30° in length. On May 12 it was still 25° long, reaching as far as δ Leporis. Between the tails were two still fainter rays, which are shown in the drawing, somewhat exaggerated in intensity.

The copy of Mr. Lunt's drawing kindly sent to the R.A.S. by Sir David Gill was too faint and on too small a scale to be satisfactorily reproduced by photography. It has therefore been necessary to enlarge it by hand *—it is hoped at no very great sacrifice of accuracy.

---

* This was kindly done by Mr. W. H. Wesley, to whom we are also indebted for these remarks.

THE BRITISH ASTRONOMICAL ASSOCIATION.

The concluding meeting of the eleventh session of the British Astronomical Association was held at Sion College, on Wednesday, June 26, *Mr. G. M. Seabroke*, the President, in the Chair.

The list of presents included 50 Vols. (xiv. to lxiii.) of *Nature* from Mr. G. F. Pollock, the King's Remembrancer.

Messrs. Henry Ellis and Gordon Miller were re-elected Auditors, and the President nominated Messrs. A. J. S. Adams, George Avenall, and T. Clapton as scrutineers of the ballot for the forthcoming election of Council.

The balloting-list for the ensuing year, proposed by the Council, was read and some additions made thereto.

Mr. Crommelin announced that the *Rev. C. D. P. Davies* had given notice that at the Annual General Meeting in October he would move an alteration of the rule providing for the loan of instruments, his proposition being that the Clause stipulating that borrowers "must have had previous experience with the class of instrument they propose to borrow" be deleted.

A letter was read which Mr. Newbegin had received from *Mrs. Maunder*, dated from Mauritius, in which she said:—"I have just time, before the mail goes, to tell you that we got 14 photographs during totality with your telescope. We have developed them all, and, without a single exception, they are exceedingly fine, and form a splendid series. Mr. Walter, of the Observatory, made the exposures. We discovered, among our possessions, dark slides that exactly fitted your instrument, and so were able to take so many." Mrs. Maunder then mentioned that her husband was suffering from an attack of malaria, which came on directly after the eclipse.

The President said he was sure all present would join in congratulating Mr. and Mrs. Maunder on their success in obtaining so many photographs, and in hoping that Mr. Maunder would soon be able to return home.

Mr. E. Holmes read a paper on "The Limits of Vision." Premising that, after many years of attention given to questions relating to vision, and especially to its limits and to alleged extraordinary instances, he had come to the conclusion that if they eliminated the cases of defective organs, the sight of the remainder of mankind was of a fairly uniform character, especially as regarded definition. The writer then proceeded to discuss the three questions:—(1) What is the smallest visible area? (2) seeing faint objects and detecting faint shades; and (3) the perception of minute detail. He maintained that there was no assignable limit to the minuteness of visible objects—it was, he said, entirely a question of contrast. If an object was sufficiently brilliant on a dark background it would remain visible, however minute it might be. But the visibility rapidly lessened as brilliancy diminished, or as the background became lighter, until the point was reached where contrast failed. He did not think it possible to lay down any exact rule regarding the perception of faint objects or faint shades. The sensitiveness of the retina was a variable quantity, like the sensitiveness of the photographic plate. The question of the perception of minute detail, or dividing and separating, was of a different character, and they had firm ground on which to form a judgment. Under this head the writer dealt in considerable detail with the possibility—or otherwise—of seeing the ellipse of Saturn, the crescent of Venus, or the satellites of Jupiter without the assistance of the telescope.

Mr. W. T. Lynn agreed with Mr. Holmes in thinking that no one ever saw the satellites of Jupiter before the telescope was invented. It seemed to be quite out of the question. The Chinese and Japanese had been said to have seen them long before; but if anyone saw a star near Jupiter with the naked eye, it did not follow that it was one of the planet's satellites.

Mr. A. Kennedy said he recollected an old lady, who had wonderful sight, plotting down the satellites of Jupiter on paper from the heavens, and when the planet was looked at through a glass it was found her positions were quite right.

Mr. T. Clapton said he remembered a young lady, who had very keen sight, remarking when he pointed Jupiter out to her, "what are those little stars very close?" At his request she plotted them on paper, and they were quite right as regarded position. He also remembered seeing the satellites of Jupiter when about 14 years of age, and some seven years ago his son and himself believed they saw a satellite of Jupiter, at any rate their observations were in perfect agreement.

Mr. G. J. Newbegin said he knew two instances of the satellites of Jupiter having been seen with the naked eye. One instance was that of a gentleman, who, upon looking at Jupiter with the naked eye, asked what the small stars on one side were, and then upon looking through the telecope said "They are on the other side now."

Mr. Crommelin said the Rev. T. W. Webb, in his book on 'Celestiol Objects for Common Telescopes,' laid claim to having seen Jupiter's two outer satellites merely with an ordinary concave eyeglass, which corrected his near sight.

Mr. S. A. Saunder said he could not help thinking that in view of the amount of short sight, long sight, and astigmatism that existed there must be a large number of eyes having these defects in smaller and varying degrees which passed as normal.

Mr. J. M. Offord remarked that Mr. Holmes had said that there was no evidence of the ancients having seen Saturn's rings; but the Chaldæans represented Saturn as a figure surrounded by a ring. This might have been pure chance, still it was a curious fact.

Mr. G. J. Burns submitted a paper entitled "The Apparent Thinning-out of the Stars." He pointed out how "indicated density" diminishes as we ascend to higher magnitudes, and said that the fact that the indicated density is always less than unity, can hardly be due to any other cause than an actual thinning-out of the stars as their distance from us increases. This conclusion was confirmed from an examination of the known proper motions of the stars.

Mr. Holmes said Mr. Burns's conclusion appeared to be based on the idea that the stars were at a somewhat approximately equal distance apart. To begin with, they knew that was not correct. Therefore the foundations of all arguments based upon this supposition were cut away. Then again, if they took the forty-one stars of the first and second magnitude, there were several of those stars that would be near the margin of second magnitude; if they were taken away and added to the second and third, they at once altered all the relative densities throughout.

The President said the subject was a very interesting one, but one in which they had to make large assumptions for bases to every argument, and there were a great many difficulties. Mr. Burns was not the first to remark that the stars apparently thinned out as they got further away from the Solar System, and the possible explanation of that was that the light of the more distant stars was not carried to us without obstruction of some sort.

The President said there were several papers which referred to the Comet recently observed, and *Mr. Chambers* desired to make a remark or two on the same subject.

Mr. G. F. Chambers, referring to an observation of his of what he had supposed to be the comet, said he should like to offer a brief explanation. Understanding from the announcements that the comet was coming North and might possibly be visible in England, he rose at 2.30 on the morning of May 1 and saw towards the eastern horizon what he would describe as a beam of light resembling a comet. His wife and two daughters all saw it also. Clearly it was not the comet in question, and it was not visible on the following night. What it was he could not say. He could only describe it as a beam of light.

Major L. A. Eddie wrote from Naauwpoort, Cape Colony, that on Wednesday evening, May 1, the comet presented a very beautiful appearance in the western sky about 20° above the horizon, when its elongated nucleus shone with great splendour, and its train of cometic matter streamed upwards for about 6° with a slight curvature and inclination to the south. On May 2, a broad, lateral brush was traceable, proceeding from the nucleus for about 5°, and making an angle of 30° with the horizon, giving a bifurcated appearance in conjunction with the more upright and brighter appendages.

Mr. Crommelin also read a note received from *Mr. C. J. Merfield*, Sydney, stating that the comet was a very fine object in the western sky. On the last few evenings, prior to the date of writing (May 6), the tail had undergone some remarkable changes, Mr. Merfield forwarded a sketch of the appearance as seen with the naked eye.

The *Rev. J. T. Bird*, chaplain to H.M. Forces, writing from South Africa, said that on the morning of April 26 an officer who had been on picket duty all night reported that he had seen a comet rise in the E. about 5 A.M. The first time the writer saw the comet was on May 2, when it was a bright object nearly due west. The nucleus was as bright as Mercury and for 3° or 4° from the nucleus the tail was very bright. On May 5 the tail consisted of two main branches, a bright branch almost straight, and from 10° to 12° in length, and a faint branch two and a half times as long, making a very acute angle with the bright branch towards the south. Later the comet became fainter, and promised soon to be invisible to the naked eye.

The Orbits of Jupiter's Satellites.

[Continued from p. 274.]

FROM what has been said already it will be clear that the numerical formulæ for the satellites' coordinates, arrived at by Souillart, are useless for discussing the eclipses; for these have already been computed for Damoiseau's tables, from which Souillart's expressions differ by large and systematic quantities. But for other phenomena these formulæ may be used; since any tables that are employed must first be constructed, and there is no more labour in constructing them from Souillart than from any other source. Such tables were framed by Mr. Marth * and published in *Monthly Notices*, vol. li. (June 1891).

These tables require some scrutiny. They are arranged so as to permit the calculation of the position of any satellite at any time. They comprise tables of the inequalities, with motions of the various arguments for intervals of 10 minutes over a range of 6 hours, and for intervals of 6 hours over a range of 10 days, together with values of the arguments at intervals of 10 days throughout the year 1891; and from year to year Marth carried on this latter portion of the table as it was required. In place of the latitude above the plane of Jupiter's orbit, the node and inclination of the instantaneous orbit are given relatively to the plane of Jupiter's equator as determined by Souillart, and the node of this upon Jupiter's orbit; when the true longitude has been computed these give the latitude very readily.

In the tables of the longitude, Souillart's smaller inequalities have been disregarded, the largest so treated having a coefficient of $4''\cdot582$ in I. and $4''\cdot208$ in IV.

In bringing up the perijovia from Souillart's epoch Marth has introduced some errors, ϖ_1 and ϖ_3 exceeding the numbers given by Souillart's formulæ by about 20° in each case.

---

* Data for computing the positions of the Satellites of Jupiter, 1891. With Tables of the Inequalities.

The formula used for the inequalities of the radius vector of II. is not in agreement with Souillart, some of the arguments being wrong; and there are some "plausible corrections" mentioned but not enumerated on p. 512. As to what these are I have no idea.

But it is in the absolute magnitude of the radius vector that Marth requires most careful examination. From eclipses alone the absolute dimensions of the system are not determined. In order to find them, both Laplace and Damoiseau employed micrometer measures of the maximum elongation of IV. and of the equatorial radius of Jupiter, the compression of Jupiter and the dimensions of the other orbits being derived afterwards by theory. The equation which connects the masses, mean motions, and mean distances may be written*:—

$$\frac{\mu}{a^3}\left\{1+\frac{A^2}{a^2}(\rho-\tfrac{1}{2}\phi)\right\}=n^2+\tfrac{1}{2}M^2+\tfrac{1}{2}\Sigma\frac{m'}{a}\frac{dA_0}{da},$$

in which

μ = sum of masses of Jupiter and satellite,
a = mean distance of satellite,
A = equatorial radius of Jupiter,
ρ = ellipticity of Jupiter,
ϕ = ratio of centrifugal force to gravity at equator,

the quantities upon the right being mean motions and perturbative terms, to which I shall not refer further.

It is a legitimate question, taking the right-hand side as known, and also the masses of the satellites in terms of Jupiter's mass, with $\rho-\frac{1}{2}\phi$ and A, to find the mean distances of the satellites in terms of any one of them or in terms of A; or again, as was done by Bessel and afterwards by Schur, to determine with heliometer observations the actual and not merely the relative dimensions of the system and then from the above equation, assuming values for $\rho-\frac{1}{2}\phi$ and the masses of the satellites, to derive the mass of Jupiter. The results of Bessel and of Schur are as follows—the assumed mean distance being 5·2073. I have added the numbers of Damoiseau, as recorded by Souillart (p. 10).

	Bessel.	Schur.	Damoiseau.
	"	"	"
a_1	111·742	111·652	111·123
a_2	177·797	177·917	176·802
a_3	283·606	283·649	282·025
a_4	498·866	499·014	496·040
1/M	1047·88	1047·23	......

To find these quantities Bessel employed the ellipticity of Jupiter and the masses of the satellites recorded in Damoiseau's Introduction, and Dr. Schur follows Bessel.

Marth's treatment of the point is curious; on p. 516 he gives a table purporting to show what mean distances correspond to any assumed mass of Jupiter from 1/1047·0 to 1/1048·4. From this we may extract, at mean distance 5·20280,

---

* Adams, Collected Papers, ii. p. 176.

	1047·0.	1047·7.	1048·4.
	″	″	″
a_1	111·787	111·762	111·737
a_2	177·858	177·818	177·778
a_3	283·704	283·640	283·576
a_4	498·876	498·866	498·755

The numbers in the same horizontal line are connected by the assumption *mass of Jupiter*/a^3=constant, as may be easily verified; but whence did Marth derive the numbers that stand in the same vertical column, and what authority is there for associating them with any particular value of the mass of Jupiter? What diameter of Jupiter and what ellipticity do they imply? It is, at any rate, certain that they agree with none of the three sets recorded above; and whatever their source may be it is clear, and it is very important to recognize, that their association with one mass of Jupiter rather than another has no authority beyond the authority of this source, which is unknown, and it is at present simply Marth's *ipse dixit*.

The work of Bessel with the heliometer, to which I have referred, was published in 1842; that of Dr. Schur in 1882; in both cases distances of the satellites from the two rims of Jupiter's disc were measured, and also position-angles. We need have no hesitation in giving an altogether different order of credit to Sir David Gill's and Mr. W. H. Finlay's observations at the Cape, in which the quantities measured were the distances of two satellites from one another and their relative position-angles; all six combinations being observed as a rule each night. These observations were made between August and December 1891, Sir David Gill observing on 35 nights and Mr. Finlay on 11. There result 550 measures, half of them being measures of position-angles and half of distance.

As to the actual observations and the method in which the difficulties inseparable from the heliometer are dealt with, I will say little; in the first place, the present publication does not include the details of observation and reduction, but only sufficient account of them to explain Mr. de Sitter's discussion, full details being promised later in Vol. VIII. Part I. of the Cape *Annals*; it would in any case be idle for me to profess to criticise them, and probably readers will be willing to accept them as equal to the best that the heliometer has done.

Absolute measures of position-angle and distance were secured by reference to two stars near Jupiter and nearly in the plane of his equator which were compared with the standard stars of the triangulation of the Victoria comparison stars; the distance and position-angle of this pair as given by the heliometer were

$$6705''\cdot36 \pm 0''\cdot065 \text{ and } 68^\circ\ 4'\ 30'' \pm 2''\cdot1,$$

while from fifteen observations upon the meridian were found

6705″·29 and 68° 4′ 32″.

This standard pair was observed on every night on which observations of the satellites were made.

While the eight pointings of which a complete observation consists are being made the satellites may change their positions by sensible amounts; this is dealt with by arranging the observation symmetrically, taking the mean of the readings and the mean of the instants at which the pointings are made.

The effect of a given error upon the deduced position of a satellite upon its orbit is not very easy to express, for it varies with the position; but we may gather a rough idea of the degree of precision possible to a single observation by seeing that an error of 0″·1 at Jupiter's mean distance from the Sun involves upon the orbits of I., II., III., IV. respectively errors not less than

185″, 116″, 73″, 40″,

or in time to

22ˢ, 28ˢ, 34ˢ, 44ˢ

of synodic motion. Hence it is clear that the very best observing will be taxed in order to surpass by much the ordinary eclipse observations. But it should be remembered that by such observations alone can the absolute dimensions be found.

I now come to the chief part of the present volume—the summary of Mr. de Sitter's discussion. Mr. de Sitter was attached to the Cape Observatory, and received from other members of the staff much assistance in the shape of checks, duplicate calculations, and other subsidiary work, but the credit and responsibility for this formidable computation rests with him, practically, alone. The discussion is in many ways admirable, and it is with great regret that I shall have to point to some errors which I consider very serious.

It is plain that a series of observations extending over a few months only cannot evaluate all the elements, since the motions of nodes &c. in which some of these elements declare themselves most clearly will not be sensible in so short a time. But a very good determination of the instantaneous elements at the mean epoch might be made, and this repeated after an interval would supply all that was wanted. Mr. de Sitter proceeds on these lines. The quantities which he proposes to find are corrections to longitudes at epoch, to instantaneous perijovia and eccentricities, nodes and inclinations, to the coefficients of a few of the most prominent inequalities, and to the mass of Jupiter. The chief difference between this choice and that of Bessel, who was aiming merely at finding the mass of Jupiter, is that he determined first from observation not this mass but corrections to assumed mean disturbances. But Mr. de Sitter's calculations are of a far more formidable character than the older ones, because each satellite being involved in equations with every other in turn he is not able to separate their elements from one another, and at the conclusion he is called upon to face normal equations involving 29 unknowns.

In forming the equations of condition, Mr. de Sitter employs Marth's tables; he points out that Souillart's data have not been correctly brought forward by Marth in some cases, but that this does not affect their utility for his purpose; also the error of the radius vector of II. which I have mentioned and a number of errata. None of these gave trouble. But Mr. de Sitter says nothing about Marth's treatment of the mean distances, and at this place I think he has made a serious error.

In all measures of the distances the mean distances—a_i—occur, and Mr. de Sitter eliminates the corrections to their assumed values by means of the equation

$$dM/M = -3da_i/a_i,$$

keeping only the correction to the mass of Jupiter as the unknown. But this elimination is only legitimate if all the four "a"s given by Marth are correctly associated with the mass to which he attaches them. I have attempted to show the position in which Marth's numbers stand in comparison with those determined with the greatest care and fulness of detail by Bessel. I should be very much suprised if any one of his four mean distances can be shown to be correctly dependent upon the attached mass; to assume such to be the case for all four is an act of faith I am not equal to. It seems to me that the corrections to all the four mean distances should have figured in Mr. de Sitter's equations of condition, for by the assumption that underlies their elimination he suppresses the possibility of correcting them, and runs the risk, which is almost a certainty, of adding his correction to Jupiter's mass to a number which may have been incorrectly adopted by Marth.

But there is another point; for the relation between M and a is not so simple as the equation above used by Mr. de Sitter would imply. If we refer to the relation quoted on p. 302 above, it will be seen that if the right-hand member is taken as known it is not the mass 1/M simply, but the quantity

$$\mu\left\{1+\frac{A^2}{a^2}(\rho-\tfrac{1}{2}\phi)\right\}$$

which varies as a^3, μ being the sum of the masses of Jupiter and the satellite considered, and the second factor changing with the satellite, and involving the quantity $\rho-\frac{1}{2}\rho$, which stands at least as badly in need of correction as does any element of the system. For what reason are these quantities dropped out of account?

I think it would be out of place to discuss the results arrived at by Mr. de Sitter at present; for unless these difficulties are met it is very hard to know what weight to attach to accordances with results elsewhere determined; I trust Mr. de Sitter may be able to throw some light on these points and to show that he has not ignored them; in that case I shall hope at some future time to return to the consideration of his results. R. A. SAMPSON.

Observatory, Durham.

Obituary.

Cuthbert Peek.—It is with deep regret that we announce the death of Sir Cuthbert Edgar Peek, Bart., of Rousdon, Lyme Regis, and 22 Belgrave Square, S.W. Sir Cuthbert, who was but 46 years of age, died on July 6 at Brighton, where he had been staying for about six months, suffering the greater portion of the time from congestion of the brain. From the outset of his illness it was feared there was but slight hope of his recovery. Deceased was the only child of Sir Henry William Peek, the first Baronet, of Wimbledon House, and for many years M.P. for East Surrey, a partner in the firm of Messrs. Peek Brothers & Co., Eastcheap, by his marriage with Margaret Maria, second daughter of Mr. William Edgar, of Eagle House, Clapham Common. Born on January 30th, 1855, he was educated at Eton and at Pembroke College, Cambridge, graduating B.A. in 1879. Sir Cuthbert Peek succeeded to the baronetcy, and the estates his father had purchased in Surrey and Devonshire, on the death of Sir Henry Peek in August 1898. From an early age the deceased baronet was a zealous student of scientific and antiquarian subjects, and in 1881 made an extensive journey in the little-known parts of Iceland. On his return he established a small observatory in the grounds of his father's house at Wimbledon and worked with a three-inch equatorial. In 1882 he went to Australia to observe the transit of Venus, taking with him a 6·4-inch equatorially mounted telescope, chronometers, and other necessary apparatus, his observing station being at Jimbour, Queensland, where for about six weeks in November and December he observed a large number of double stars and clusters, paying particular attention to the nebula surrounding η Argûs. The transit of Venus was not seen, owing to clouds, but Sir Cuthbert often remarked that this caused him little regret, for he felt amply repaid for the journey by his observation of the wonders of the southern sky. After extensive travels in Australia and New Zealand, he returned to England in August 1883, bringing with him a considerable collection of curious and interesting objects, which further enriched the famous museum established by his father at Rousdon.

The Rousdon Observatory was erected in 1884, and is a substantial building, standing east and west, containing on the ground floor, a transit-room on the east, and a computing-room and library on the west, the centre being occupied by photographic dark-room and chemical laboratory, and over this is the circular equatorial room, covered by a 16-foot copper dome. Here is mounted the 6·4-inch Merz refractor which has been used in the observations of variable stars for the last 16 years. At the beginning of this research in 1886 the literature on the subject was very scanty, and star-charts and lists of comparison stars (such as now make observation easy) were very rare, the R.A. and declination being in most cases the only information obtainable, and some of the long-period variables were only identified after months of patient watching. About 25 long-period variables have been regularly observed, and as most of these are circumpolar in this latitude their light-variations are continuously recorded. Up to the end of 1900, the magnitude determinations numbered 6809, by the method of Argelander, each observation consisting of 5 comparisons with known stars in the same field 267 maxima and 227 minima have been observed. The results have appeared from time to time in various periodicals, and five pamphlets of variable star notes have been published in which the light-changes of ten variables have been dealt with and illustrated by diagrams of light-curves.

Sir Cuthbert was fully alive to the value of continuous work on a definite system with the same instrument and observer, and was much gratified that the work had thus gone on at Rousdon for so many years. He had no faith in the practice of smoothing curves, but was convinced that the irregularities in many of the variables were real, and, though our knowledge of the cause was now slight, believed that some day a complete explanation of these singular changes will be discovered.

The meteorological department was founded in 1883 with the usual instruments of a second order station. It has been gradually extended by adding evaporation tanks, sunshine recorder, barograph, registering pluviometer, and various patterns of anemometers, for which a separate iron tower has been erected. Seventeen annual volumes have been published and weekly and monthly weather notes have been supplied to the local papers for many years past.

The Observatory was often open to visitors, and the genial baronet was never happier than when showing to delighted friends the wonders of the heavens, or explaining to interested enquirers the construction and working of the complicated meteorological apparatus in use for the weather records. Sir Cuthbert was a Fellow of the Society of Antiquaries, Hon. Secretary of the Anthropological Society, on the Council of the Royal Meteorological Society, and also on that of the Royal Geographical, which Society he has endowed with a medal for the advancement of geographical knowledge. Another of his interests in life may be inferred from the fact that he presented a challenge-cup and an annual prize to be shot for by the members of the Cambridge University Volunteer Corps. He married in 1884 the Hon. Augusta Louisa Brodrick, eldest daughter of Viscount Midleton, and sister of Mr. St. John Brodrick, the Secretary of State for War. He leaves two sons and four daughters. His elder son, Wilfrid, who succeeds to the baronetcy, was born on October 9th, 1884.

C. GROVER.

TRUMAN HENRY SAFFORD.—It is announced that Prof. Safford, of Williams College Observatory, Massachusetts, a distinguished

American representative of Astronomy of the older school, died on June 13 last at the age of 64.

Truman Henry Safford began his astronomical career at Harvard College Observatory under G. P. Bond the second Director, and in the years 1860 to 1863 he was not only observing diligently but was also employing himself in computing orbits of comets and planets and in making researches of a high class. Specially may be mentioned his investigation of the proper motion in declination of Sirius, which added strong confirmation to the views entertained by Bessel and Peters respecting the source of similar irregularities in right ascension, that the variations of proper motion might be due to the presence of a neighbouring companion of considerable mass. The subsequent discovery of a companion to Sirius by Alvan Clark is well known. In March 1863 Mr. Safford was made Assistant-Observer, the second position in the Observatory, which was mentioned in the Report of the Committee of Overseers for the year 1863 in these words:—"The early development in this young man of mathematical power of a very high order joined to a zeal in the prosecution of original astronomical inquiries scarcely less remarkable, renders this appointment a peculiarly acceptable one. During the last year, in addition to other pressing duties, he has determined the right ascension of seventeen hundred stars and the declination of four hundred and fifty with the injured circle, whose results, as we have often had occasion to say, can only be made of value by reductions involving much labour and time." Prof. Bond died on 1865 February 17, and the report for the year 1864 was prepared by Mr. Safford. Prof. Bond at the time of his death was engaged in the preparation of his monograph on the Nebula of Orion, which was completed by Safford and forms Vol. V. of the Harvard Annals.

In 1862 an Astronomical Society had been established in Chicago, whose members had made efforts to establish an Observatory. The effect of these was the Dearborn Observatory, built on the grounds of the Chicago University, whose Great Equatorial with an object-glass of 18½ inches aperture and 23 feet focal length was set up in May 1864. T. H. Safford was appointed the first Director of this Observatory. Here during the first three years of his office he devoted most of his time to the observation of nebulæ, discovering about one hundred not previously known. In 1868 a meridian circle was added to the equipment, and the Observatory took part in the formation of the 'Astronomische Gesellschaft' Catalogue, zone 35° to 40° N. declination being alotted to Dearborn*. The expenses of this observatory were borne entirely by private munificence, and the paralysis caused by the great fire of Chicago in 1871 deprived Safford of his support, and for the next five years family cares made it necessary for him to attend to a more remunerative employment, and he did geodetic and astronomical work for the United States engineers who were conducting extensive operations in the central portion of the country. In the year 1876 he accepted the Field Memorial Professorship of Astronomy in Williams College, Williamstown, Mass., where, in addition to the duties of his professorial chair, he has made many investigations relating to stellar astronomy of position. Specially may be mentioned a paper on "Certain groups of Stars with common Proper Motions," published in the *Monthly Notices*, vol. xxxviii., also a discussion of the lengthy and valuable observations at Greenwich under Maskelyne and Pond, for the purpose of deducing corrections to the places of the planets observed by them to make them comparable with modern systems. His conclusions as to the merits of these astronomers as shown by their work are interesting. Prof. Safford took much interest in all such corrections and small details relating to fundamental astronomy, and wrote several valuable memoirs on Personal Equation, the latest possibly being "The Psychology of the Personal Equation," published in the American *Science* for 1897 November 26, in which he compared the numerical results for "reaction-time" as found by psychologists with the lengths of the equation found by astronomers. As Director he published star catalogues made from observations at the Williamstown Observatory. A comparison of one of these—that of the Polar Right Ascensions for 1885 with the Greenwich 1880 Catalogue—will be found under his name in the *Monthly Notices*, where there are many similar papers by him. He was elected an Associate of the Royal Astronomical Society in May 1866.

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* This zone is now being finished by the Lund Observatory.

H. P. H.

Adolph Christian Wilhelm Schur.—We have been informed also of the death, which happened on July 1 last, of Dr. Schur, Director of the Gottingen Observatory, at the comparatively early age of 55.

Wilhelm Schur was born at Altona in the year 1826. He joined the Kiel University in the year 1863, and in the year 1866 proceeded to Gottingen, where two years later he received the Doctor's degree for a computation of the orbit of the double star 70 Ophiuchi by the use of Klinkerfue's formula. After this he spent some years at Berlin, part of which he spent in helping Dr. Auwers in his new reduction of Bradley's observations, and during the latter part of the period was an Assistant in the Geodetic Institute. He went with the German expedition under Prof. Seeliger to the Auckland Islands, to observe the Transit of Venus of 1874. In 1873 it was determined to establish an Observatory at Strasburg, with Dr. Winnecke as Director, and Dr. Schur was appointed his Assistant, a post which he filled with zeal and energy. One of the four heliometers which had been used by German expeditions in the Transit of Venus was subsequently transferred to this Observatory, and observations with this and the other three instruments gave Dr. Schur material for an able determination of the mass of Jupiter*. In 1882, Dr. Winnecke was compelled by extreme ill-health to resign his post and Dr. Schur was appointed to succeed him. His talent as Director may be judged from certain papers in the *Astronomische Nachrichten* for 1883 and 1884, in which he discussed in a masterly manner certain features, such as pivot-errors, flexures, and collimation, of the Repsold meridian circle of the Observatory. In 1886 Dr. Schur accepted the Directorship of the Royal Observatory, Gottingen, where the large heliometer afforded him work suitable to his taste. He has made and published careful and accurate triangulations of the stars in certain clusters, such as the Præsepe and the groups around h and χ Persei, one of which was recently reviewed in these pages †. Dr. Schur was elected an Associate of the Royal Astronomical Society in 1898 Dec. The science loses a hard and painstaking worker by his somewhat premature death.

ERNST LAMP.—We also have to announce the death, on May 10 last, of Dr. E. Lamp, which took place at Ruanda, on the East Coast of Africa. Dr. Lamp will be remembered for his contributions to the *Astronomische Nachrichten*, for his cometary observations and computations, and his reduction of meteorological observations. From 1874 to 1877 he was at the Geodetic Institute in Berlin, from 1877 to 1897 at the Kiel Observatory. Returning to the Geodetic Institute in 1897, he was sent out on the German Boundary Commission in East Africa in 1900. Dr. Lamp was born in 1850.

C. M. GAUDIBERT.—The recent death of this distinguished French selenographer is thus referred to in the Bulletin of the French Astronomical Society:—"This Society and astronomy in general has just suffered a great loss. One of the founders of this Society, the painstaking selenographer Gaudibert, of Vaison (Vaucluse), ended a life entirely devoted to doing good and to science, on Sunday, June 9. He was born on 1823, March 4."

PETER GUTHRIE TAIT.—Though he was scarcely an astronomer, it may not be considered out of place to record here the death of Professor Tait, of Edinburgh University, who died on July 4 at the age of seventy years.

Tait was Senior Wrangler in the year 1852, being then only twenty-one years of age, and in 1854 was appointed Professor of Mathematics in Queen's College, Belfast. In 1860 he was elected to the Chair of Natural Philosophy in Edinburgh University, which he occupied until February of this year, when he was compelled by illness to resign his office. Prof. Tait will be remembered as the author of many mathematical books. Thompson and Tait's 'Natural Philosophy,' of which he was the author in combination with Lord Kelvin, is classic.

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* Referred to by Prof. Sampson on p. 303.

† 1901, February, p. 97.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Perseid Meteors again.

GENTLEMEN,—

In my letter printed in your number for August 1897 (vol. xx. p. 318) I pointed out that, so far as is known, Forster was the first person to notice the fact of an annual recurrence of meteors about the 10th of that month. But it would seem that the earliest record of an actual appearance of that shower, with mention of its exact date, is by Sir William Hamilton at Naples, in the year 1779 (*Phil. Trans.* vol. lxx. p. 63). It is contained in a paper on the recent eruptions of Mount Vesuvius; a great number of meteors, he says, were noticed on the evening of the 9th of August, and it seems to have been thought that these were in some way connected with the eruption. But earlier appearances were probably recorded, and it would be of great interest and importance if their dates could be determined even approximately, so as to ascertain, if possible, whether these undergo any change in the course of centuries similar to that known to take place in the Leonid stream. Unfortunately, in the case of the Perseids the question is further complicated by the circumstance that we are several days passing through the stream, and in early incomplete records it is not easy to fix the date of the appearance of the central part.

My attention has been redirected to these bodies (now again approaching us) by a letter from M. Antoniadi, of Juvisy. He refers to a passage in Cedrenus describing a remarkable meteoric display which was noticed at Constantinople early in the reign of Justinian. It appears to me evident that this is the one referred to by Chladni ('Ueber Feuer-Meteore,' p. 88), who does not give his authority, but writes:—

"Im Jahre 533 oder im 5ten Jahre Justinians, wo man sie vom Abend bis an den Morgen in solchen Menge sah, dass es grosses Schrecken erregte und man glaubte nie etwas so Wunderbares gesehen zu haben."

The expression of Cedrenus is that there was so great a rushing of the stars that all persons were frightened and exclaimed "The stars are falling!" but knew not what it portended.

Kaemtz quotes from Chladni in his 'Meteorologie' (vol. iii. p. 231) and Quetelet in his well-known 'Catalogue des principales Apparitions d'Etoiles filantes' (p. 27), both giving the date as A.D. 533. But I think I can show that the display really took place the year before. Justinian ascended the throne in April A.D. 527; the sedition at the circus or hippodrome, known in history as the Nika, broke out in the fifth year of his reign, and as the month was January, this was in A.D. 532. Cedrenus states that the star-falling was in the same year, so that it must have been (if later than April, which is practically certain) in the sixth year of Justinian (not the fifth, which is simply a mistake of Chladni, followed by Quetelet) and in A.D. 532 *. The probability, then, is very great that it was the same display noticed in China, which took place, according to Biot, on August 28 in that year.

Now if these were Perseids, and if we could rely upon the date assigned, it would seem to indicate that the shower takes place at present about three weeks earlier than it did fourteen centuries ago; but to draw such a conclusion would be very precarious in the present state of our knowledge, especially in view of Sir William Hamilton's observation. Quetelet quotes from the first volume of the *Comptes Rendus* † a statement that a display was seen in the month Redjeb in the year 1029. In his first paper he says that this was in the month of August, but in his second he adds that, according to Herrick, that month began on the 16th of July. The Mohammedan calendar had not then begun, and by the old Arabic calendar (the names of the months in which were afterwards retained, but the chronology became quite lunar) Redjeb or Rajeb was the seventh month in the year, and probably included part of our July and only a part of August, so that the meteoric display would not be later than the middle of that month.

Perhaps the above remarks may impart some additional interest to a careful series of observations of the Perseid meteors on their forthcoming return. They are at any rate more constant friends than the Leonids, and as even on the 10th of August this year the Moon does not rise till past midnight, she will not much interfere with their observation. Yours faithfully,

Blackheath, 1901, July 2. W. T. Lynn.

Motion of the Red Spot on Jupiter.

Gentlemen,—

The following estimated transit times of the red spot have been made here since the early part of September 1900:—

			T.T. h	T.T. m	Longitude. °
1900	September	3	7	10	44·4
1901	February	13	17	37	43·4
	May	1	16	21	46·4
		23	14	24	43·6
		28	13	35	45·9
	June	14	12	34	45·9
		24	10	46	44·7
		26	12	28	47·1

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* In Quetelet's second paper ('Nouveau Catalogue,' 1841) he says that Herrick had suggested this as the more probable year.

† The reference he gives to p. 293 is certainly erroneous, and I have not been able to find the place.

		T. T.		Longitude.
		h	m	°
1901 July	4	9	0	44·6
	8	12	17	45·2
	18	10	32	45·6
	20	12	9	44·8
	28	8	49	46·5

The first two observations were obtained with a 4-in. Cooke refractor, power 240, the remainder with a 10-in. Browning reflector, power 312.

Between 1898 March and 1900 September 3 the longitude of the marking increased from about 23° to 44°, or at the monthly rate of $0^\circ{\cdot}7$, relatively to Mr. Crommelin's system II. based on the period $9^h\ 55^m\ 40^s{\cdot}63$. During the 2½ years included in the period referred to, the rotation period of the spot was therefore

$$9^h\ 55^m\ 41^s{\cdot}6.$$

But from 1900 September 3 to 1901 July 28 the easterly drift in longitude has amounted to about 2° only, which corresponds with a rotation period of

$$9^h\ 55^m\ 40^s{\cdot}9.$$

So the increase of velocity, relatively to its rate in the few preceding years, seems to have been well marked during the last 10 months.

Your sincerely,
W. F. Denning.

Bishopston, Bristol,
1901, July 29.

Anomalous Occultations.

Gentlemen,—

From the notice which my paper on this subject receives on page 210 of your valuable journal, I fear that I must have failed to express my ideas.

I had no intention of referring to the phenomenon of stars "hanging on the limb" or "projection on the disk" of the Moon, as such is, in my opinion, of no cosmical interest whatever, it being an optical or subjective effect, which, in my own experience, can be produced at any time by ill-focussing. I have practically no experience of bright-limb occultations, but should think that such are more liable to "projection" than dark-limb occultations.

The phenomenon that I desire to draw attention to is of quite a different order, and would still exist in spite of, or even along with, "projection."

If a star is a wide double, so that each component can be clearly seen, it is quite evident that these components will in general be occulted at different times. What will be the effect if we imagine these components to be lessening their distance apart? Clearly that the difference of times of separate occultation will get less and less until it becomes too small to be recognized as due to two phenomena. Thus a double star can disappear in two distinct stages, or it may glide out more or less slowly, or disappear instantaneously, which is the usual manner, in which case it cannot be distinguished from a single star.

As such, this subject has only been meagrely discussed in the past. The long papers by South and Airy and others seem to have no bearing whatever on it. As far as my cursory examination goes, it has only been referred to on a few occasions, viz.:— (1) Burg's observation of the occultation of Antares, where Burg clearly fathomed what he had seen, but was pooh-poohed out of it by Bode's learned but erroneous explanation; (2) Barnard's observations confirmed by Burnham; (3) Tebbutt's and another's observations of τ_1 Arietis.

In looking over the long lists of occultations of Aldebaran (which seems to be by far the most observed star at occultations), the records in no case give any indication of duplicity; but projection on the disk is common, probably due to the telescope having been focussed on the red star, whereby the Moon will be slightly out of focus.

It is very seldom that any old record of a star gliding or fading away or disappearing in two stages occurs. With Spica I have so far only come across one fair case, viz.:—

"T. W. Burr, 1857, May 6.—Spica appeared to emerge more in the leisurely manner of Jupiter's satellites in January last, than instantaneously as the stars usually do." (*M. N.* xvii. p. 203.)

One positive case must be allowed to outweigh many negative cases—firstly, because, if the position-angle of the double star is tangential to the Moon's limb at contact, the occultation will still be instantaneous; this may be modified by irregularities of the Moon's limb, whose effect is here at a maximum: secondly, it is not every observer who takes the trouble to record minutiæ.

Spica when undergoing occultation in the most favourable conditions as to distance of its components and their angle to the Moon's limb may still be so close that the successive intervals appear as one to the human mind; but there are doubtless many other double stars intermediate between Spica and doubles of too long a period to be discovered by spectroscope, yet too close to be discovered visually, that may reveal themselves by the magnification due to the Moon taking an appreciable time to traverse a space too minute for direct measurement.

I would like to draw attention to the valuable series of occultations by Mr. Tebbutt. His notes may well serve as models to others. I believe the conscientious observer has a pleasant and useful field open to him in the careful observation of dark-limb occultations by merely noting in what manner the star disappears. These can be looked for every clear evening between two and eight days of the Moon's age. No predictions are necessary.

Yours faithfully,
R. T. A. Innes.

Cape of Good Hope,
1901, May 25.

P.S.—Since writing the above I have been favoured with a letter from Mr. C. L. Brook, of Meltham, Yorks, who quotes two striking cases of anomalous occultation, which I relate almost in his own words:—

> χ_1 Orionis.—I observed the disappearance and reappearance of this star on 1900, Nov. 9. At disappearance I could not follow the star quite up to the Moon's limb; but at reappearance, which took place at the dark limb, I made the following note:—The reappearance was not instantaneous, but took place in two distinct steps, separated by an interval not far from ½ second. I afterwards read in his 'Handbook of Astronomy,' edition 1889, vol. i. p. 358, that Mr. Chambers, 1863, Oct. 30, had observed the emersion of this star (it is printed ψ_1 by error in the Handbook) not to be instantaneous.
>
> D.M. $+18^{\circ}\cdot1349$.—On 1901, April 23, I noted:—"This star was occulted in two portions. About ½ of the light disappeared first, and after an interval, estimated at not less than 2 seconds, the remainder disappeared instantaneously." Afterwards I identified the star as the binary OΣ 156.

As to the case of κ Cancri, mentioned by Mr. Knobel at the meeting of the R.A.S., Dr. Copeland wrote about the occultation of that star on 1863, April 26:—

> About three quarters of the light disappeared in the usual instantaneous manner, and after an interval of (as near as I can judge) rather more than half a second the remaining portion disappeared.

Mr. Chambers writing about this says:—"Dawes regarded this as a decisive indication that the star was double, though he failed to verify the surmise." It is also proper to remark that the same occultation of κ Cancri was observed by Mr. T. W. Buer, and recorded as "instantaneous"; but, as I have already pointed out, a really double star does not necessarily disappear in two stages if it is a wide pair.

[Those who have read Mr. Innes's paper in the April number of the *Monthly Notices* will certainly not think that he has "failed to express his ideas"; but as the paper was not read *in extenso* at the meeting, and Mr. Knobel's subsequent remarks were made, and our note was written, with reference to the abstract read to the Society, in which the word "gliding" did not occur, Mr. Innes has, perhaps, reason for feeling that a wrong construction has been put on his words. The distinction between the phenomenon "hanging on the limb," mentioned in our note, and the "disappearing in two stages," which occurs in Mr. Innes's paper, is, however, so subtle, that we still think, as when our note was written, that he would be well advised to include these in his collection. Sir George Airy's memoir, we believe, included anomalous occultations of this kind.

As to the second paragraph of Mr. Innes's letter, it seems necessary to say that in our opinion no astronomer ought to tell another astronomer that the phenomenon he records is due to ill-focussing. It reminds one a little of the scientific society on the Stanislaus, which, as Bret Harte relates, ended in the members throwing the old red sandstone at one another; and, besides, it is always possible that the other man can handle his telescope as well as the man who makes the charge. With regard to Mr. Innes's opinion that the phenomenon known as "projection on the disk" is a subjective or optical effect (which has been previously suggested), I am glad to take this opportunity of calling attention to a little piece of evidence. On some occasions, the most recent of these being the Lunar Eclipse of 1899, Dec. 16, I, with others, have observed many occultations at one sitting, under the same conditions of person and of telescope, and I have on these occasions seen stars disappear and reappear, some suddenly, others projected on the disk, and others apparently gliding behind or from behind the Moon. It will be seen from the records of these * that these different kinds of occultation appear promiscuously in order of time, and I believe they have no connection with the colour of the star. I think, but I make the remark with reservation, that they have some connection with the place on the limb where occultation takes place, but the facts certainly seem to contradict the assumption that the phenomena are subjective.—H. P. H.]

PUBLICATIONS.

RADCLIFFE OBSERVATIONS, VOL. XLVIII.: EARTH-THERMOMETERS.—An important advance due to the late Mr. Stone has recently been made in the investigation of earth temperature, by the adoption, at the Radcliffe Observatory, Oxford, of five platinum-resistance thermometers (of the Callender and Griffiths pattern) for this class of work. The first four thermometers, mounted by Mr. Stone shortly before his death, were placed at the depths, approximately, of 6 inches, 1 foot 6 inches, 3 feet 6 inches, and 5 feet 8 inches. The fifth thermometer, which Mr. Stone intended to have had placed at the depth of 20 feet, was obliged to be mounted at a depth of ten feet only, on account of water having been met with at a slightly lower level. It is to be regretted that it was not found practicable to mount similar thermometers at greater depths than 10 feet. The thermometers are buried in practically unbroken ground, being inserted in iron pipes driven horizontally in undisturbed gravel, a temporary pit having been dug to facilitate the placing of the thermometers and the necessary wires in position. The standardizing of the ther-

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* The observations during the eclipse above mentioned are given in the *Monthly Notices* for 1900, January. On referring to my note-book of original observations I find that four stars are noted as having been seen within the limb before disappearance, in five cases the phenomenon happened suddenly, in three the star glided under the Moon, and in one case it appeared to glide from under. It is to be regretted that for some reason these last four cases were not noted in the *Monthly Notices*; but to the three disappearances to which no note is assigned, and to the reappearance of B.D. 22°·1000, the word "glided" should be added.—H. P. H.

mometers was performed by Dr. Rambaut after taking up his appointment as Radcliffe Observer, in accordance with the method described by Mr. Griffiths, F.R.S., in *Nature*, Vol. liii. p. 39; and after a great number of experiments had been made for the elimination of errors and discrepancies, the thermometers were brought into regular action on 1898 November 1. The following table of monthly mean temperatures of the ground, as given by these thermometers, is extracted from the 'Radcliffe Observations,' Vol. xlviii. p. xv.

Mean Monthly Temperature of the Ground at the Radcliffe Observatory, Oxford, 1899.

Thermometer ... Depth...............	I. 6½ in.	II. 1 ft. 6 in.	III. 3 ft. 6½ in.	IV. 5 ft. 8½ in.	V. 9 ft. 11½ in.
	°	°	°	°	°
January...........	40·47	42·07	44·68	46·80	49·97
February	40·09	41·34	43·25	45·08	48·33
March	41·34	41·91	43·21	44·74	47·42
April	48·77	47·66	46·61	46·40	47·37
May	54·86	52·95	50·96	49·54	48·53
June	66·73	62·89	58·29	54·73	50·88
July	69·43	65·93	62·15	58·74	53·85
August	69·23	67·79	64·88	61·66	56·39
September........	59·34	61·12	62·03	61·19	57·80
October...........	48·99	51·14	54·29	56·12	56·71
November	46·73	48·43	50·99	52·69	54·48
December	38·14	41·08	45·35	48·58	52·33

It would appear that the claim advanced by Dr. Rambaut, that "the method of platinum thermometry seems to be particularly suitable for this class of work, on account of the immunity it enjoys from certain errors attending the use of the long-stemmed spirit thermometers ordinarily employed for underground temperature," is well founded.

It may not be inappropriate to add one or two words with regard to the older series of observations with *long-stemmed spirit thermometers,* which have been incidentally referred to. It appears to have been a convention with meteorologists in early days to sink their earth thermometer-bulbs to the depths of 3, 6, 12, and 24 French feet below the surface (say 3·2, 6·4, 12·8, and 25·6 English feet). The earliest set of thermometers installed at these depths seems to have been that of Brussels in 1834, followed by Edinburgh in 1837, and by Greenwich in 1846. The Brussels observations from 1834 to 1842 have been discussed by Quetelet the Edinburgh observations by Thomson, Everett, and Piazzi Smyth, and those at Greenwich by Everett and Airy.

The Greenwich observations are still being carried on with the original (1846) thermometers, but the Edinburgh thermometers were destroyed in 1876, and were not renewed until within a year or two ago by Dr. Copeland; and nothing is known about the fate of the Brussels thermometers.

There are two great faults, at least, attending the use of these spirit thermometers, the most obvious fault being the impossibility of determining their errors, which in some cases may be considerable and may increase with age. Another fault in this method is the excavation of the ground for the placing of the thermometers and the filling up the pit with dry sand, which, being of a more or less porous nature, allows the percolation of storm-water to the bulbs of the 3 feet and 6 feet thermometers, consequently vitiating the indications for a time. W. C. N.

RESEARCHES UPON PRECESSION AND SOLAR MOTION.—In No. 501 of the *Astronomical Journal*, Professor Lewis Boss gives a very interesting account of the difficulties attending the determination of the Precession Constant and the Solar Motion in Space. As this subject may be found an interesting one to the general reader, we take the opportunity of extracting some of the more readily intelligible parts. The attempt to find the point in the sky toward which the Sun's motion is directed is difficult, not only because of the hypothetical nature of the assumptions which must be made, but also because of the fact that the parallactic motion of the stars is entangled in a perplexing way with other effects and motions. Some of these arise from the imperfection of observations, others from the unknown residuum of precessional motion, and still others from the motion which is peculiar to each star. We cannot be sure whether this *motus peculiaris* is principally a matter of chance. That there is community of motion in groups of stars we know. There is a notable group in Ursa Major, another in Taurus. Admitting these star-streams may be more prevalent than is generally supposed, the hypothesis of chance motion may still hold provided the star-stream instead of the single star be taken as the unit. What we really do is to assume that, if we divide the visible universe into volumes, each containing a large number of stars, the resultant of the combined motions in each unit of volume must be the same in velocity and direction as that of the entire volume made up of those units. Now, if in any area of the sky we consider a large number of stars, all at distances from the Sun not greatly differing from each other, it is obvious that the mean of all their proper motions will approximate to the parallactic effect for a star situated at their mean distance. The next question is to settle the limits of this distance. For the larger proper motions we may fairly assume the proper motion itself a good measure of relative mean distance; but when we pass below proper motions of 10″ per century this criterion begins to fail, until for motions of 2″ or 3″ it is useless. Yet it is this class of stars on which we must eventually rely for determinations of the precessional constant and solar motion, because it is the only class where the numbers are sufficiently large to afford reasonable ground that the *motus peculiaris* can be virtually eliminated. Fortunately, the brightness of the stars, considered in large aggregations, also affords a rude criterion of distance, so that generally the mean proper motion may be said to decrease with the brightness. If, then, we include only stars of the seventh magnitude, or brighter, Prof. Boss finds from his researches that we may be reasonably confident that for the great majority of the stars the relative distances will be included between 1·0 and 3·0, where 1·0 expresses the distance of the stars with a proper motion of 15″ per century. Indeed the conviction impressed upon him by reflection upon these facts is, (1) that the Sun appears to be situated within a star-cluster of tolerably definite outlines; (2) the dimensions of this star-cluster may be stated by estimating the annual parallax of stars brighter than the seventh magnitude to be in very few instances less than 0″·003, and of the ninth magnitude less than 0″·002. Using the Cape Catalogues of 1850 and 1880, Prof. Boss obtains for the apex of Sun's way

$$A = 242^\circ{\cdot}9, \quad D = +43^\circ{\cdot}4.$$

By treating these in a somewhat different manner he deduces

$$A = 263^\circ{\cdot}7 \quad \text{and} \quad D = +45^\circ{\cdot}9.$$

He then takes Ludwig Struve's determination made from Auwers-Bradley and the Pulkova Catalogue of 1855, and, after correcting for systematic error, obtains

$$A = 267^\circ{\cdot}0 \quad \text{and} \quad D = +42^\circ{\cdot}8.$$

Having done this he combines the corrected Struve with his own and deduces a final result of

$$264^\circ{\cdot}6 \quad \text{and} \quad +44^\circ{\cdot}4.$$

In a similar manner he corrects other determinations to Newcomb's epoch, and finally from the consensus of all investigations the results for solar motion may be presented in the following form:—

Stars.	Mag.	A.	D.
Small proper motions	8·5	279°	+49
Small proper motions	6·0	265	+44
Large proper motions	—	280	+45

From these he adopts as the most probable values

$$A = 275^\circ \quad \text{and} \quad D = +45^\circ.$$

That is, a point about 7° in a north-westerly direction from α Lyræ.

NOTES.

Comet Notes.—It appears that the recent comet was first seen by Viscara at Paysandu, Uruguay, on the morning of April 12, 11 days before the other discoverers. This comet, like that of 1882, has had so many independent discoverers that it will probably be known simply as the Great Comet of 1901, without any further name attached. According to Major Eddie (South Africa), the comet was just visible with a binocular on June 9 and 10, so that it is probably now altogether invisible.

Prof. Bredichin has written an article on Comet 1899 I. (Swift) (Bull. de l'Acad. Imp. des Sciences de St. Pétersbourg, May 1901).

The comet was discovered on March 13, 1899, by Lewis Swift and passed perihelion a month later.

Before this it had a nucleus of the 10th mag., a coma some 7′ in diameter, and a feeble tail.

After perihelion it became visible to the naked eye, reaching the 3rd magnitude, its tail being several degrees long.

On May 7 the nucleus was observed double at Lick, and the two portions moved wider and wider apart, being 29″ apart on May 21. The fainter component was followed till June, when it was lost through faintness.

Many photographs of the comet were taken at Lick during May and June. Sketches from some of these are reproduced, one being extremely like a drawn sword or scimitar, explaining the fanciful drawings of comets which abounded in the Middle Ages. They have a curious twisted aspect, suggesting rotation or oscillation about the line drawn from the Sun to the comet.

He remarks that the tail was of type I., with the exception of a faint streamer, which was of type III. He has been able to trace on the photographs the outward and vibratory motion of the material of the tail. Thus he says that the extremity of the tail on May 19 left the head 4 days earlier, or on May 15.

He says that the partition of the nucleus was caused by the disturbing action of the Sun. Both nuclei were moving in hyperbolas, the smaller having the greater eccentricity.

He points out that elliptical orbits may be formed from parabolic ones not only by planetary perturbations, but by the action of the Sun causing disruption of the nucleus, some portions being thrown into elliptical orbits, others into hyperbolic ones. Many examples of such disruption are now known. A. C. D. C.

MINOR PLANET NOTES.—A new planet, GP (mag. 11·8), was discovered by Carnera at Heidelberg on July 12.

The following planets have been named:—

393 Lampetia.
399 Persephone.
407 Arachne.
408 Fama.
415 Palatia.
417 Suevia.
418 Alemannia.
419 Aurelia.
450 Brigitta.
455 Bruchsalia.
GJ Kilia.

The planet (345) Tercidina has been suspected of being variable like Eros, with a period of between 4 and 5 hours. Recent photographic trails show that the variation is now very small. This, however, is quite conformable to the analogy of Eros.

A. C. D. C.

THE ECLIPSE OF MAY 18.—Letters from Mr. Maunder to the Astronomer Royal give further information about the Mauritius expedition, which, as regards the writer's health, is not altogether pleasant reading. The letter on May 26, mentioned in our last number, relates that Mr. Maunder had apparently got over the fever, but was left in a state of weakness; a few hours after this was written the fever returned in a much severer form, so that he was not able to leave his bed for a week, and then, on Monday, June 3, had to go to Curepipe, the highest ground in the island. Writing on June 9, however, Mr. Maunder says:—"I have made good progress, and am now able to take regular food, and have recovered something of my usual strength."

Under these circumstances the developing of the eclipse photographs has had to be left almost entirely in the capable hands of Mrs. Maunder and Mr. A. Walter, the Chief Assistant of the Observatory. The report at the date of writing (June 9) is as follows:—

With the Mauritius photoheliograph three photographs were obtained during totality. These are poor, owing to the excessive atmospheric tremors. The partial-phase photographs—34 exposures—give the clearest evidence of the extreme agitation of the air and of the perfect focussing of the instrument.

The coronagraph (4-inch photoheliograph fitted with a negative enlarger to take an image of the Moon two inches in diameter) secured 7 photographs during totality. These are distinctly better looking images than those taken with the photoheliograph. The same cause has prevented the definition being good, but the magnification being smaller the results appear much better.

The rapid rectilinear lens (4-inch Dallmeyer of 32-inch focus) yielded 6 photographs. These show Mercury and Venus well, and very considerable extensions.

The prismatic camera (lent by Mr. Evershed and used by him in India—2-inch aperture) gave no results.

Mrs. Maunder's photographs were numerous and, so far as yet developed, very successful. In particular, a set of 14 photographs of the corona taken with Mr. Newbegin's telescope are very fine. This instrument, being considerably higher than those fed by the coelostats, was not quite so seriously affected by the excessive "boiling."

Mr. and Mrs. Maunder were, at time of writing, practically "prisoners of war" in Mauritius, as the "Messageries" line of vessels "quarantined" the island. Owing to Mr. Maunder's illness it was impossible for them to leave by the British India line boat which brought the letters, and the next vessel of that line would not, in the regular course of things, leave until July 12. Mr. Maunder therefore hoped to be in England before the middle of August.

Mr. Dyson is expected to be home on August 1.

TYCHO BRAHE'S TOMB.—The following translation of an article from the *Neue Freie Presse* of Vienna appeared in *Nature* of July 11:—

> On the occasion of the 300th anniversary of Tycho Brahe's death the Prague Town Council decided to gather together the remains of the celebrated astronomer, which were in the Teyn Church, and bury them anew. Under the guidance of Mr. Herlein this operation was commenced yesterday. After having lifted the stone block on the monument, which is situated near the first column in the nave, and which bears a full-length effigy of the great astronomer, a semi-collapsed arch was found, and on removing the stones two mouldering coffins were seen. On the following day a committee met to determine whether these bodies were those of Tycho Brahe and his wife. Two workmen with candles descended into the vault and removed the débris which covered the coffins, the wood of which was quite rotten and fell to pieces at every rough touch. About 10 A.M. the lid of the first coffin was free to be removed. It was a surprising sight that met the eye; the body in the coffin was a wonderful likeness of the effigy on the monument. The head was slightly turned to one side, the bones of the face and the peaked Spanish beard being well preserved. The head was covered with a skull cap, and the neck was surrounded by a Spanish ruff which, like the remainder of the clothing, had suffered little during the 300 years since Tycho Brahe was laid in his last resting place. The feet were shod in long cavalry boots reaching up over the knee. That the body was Tycho Brahe's was also seen from the absence of the nose; Tycho lost this organ in a duel and wore a silver one in its place. Amongst the rubbish was found a silver wreath and spray of flowers. The construction of the grave was rather remarkable, the stones being laid loosely over one another. This is all the more astonishing seeing Tycho Brahe was buried with great pomp and honours, but it is supposed that the vault broke down during the restoration of the church in 1721.

Dr. Dreyer writes to us that on further examination the "long cavalry boots" turned out to be nothing but accumulated dust, which was due to the vault having fallen in, probably in 1721, when the chancel was re-flagged. He adds that the Academy of Sciences in Copenhagen is bringing out a reprint of Tycho's little book on the star of 1572.

AWARDS AT THE PARIS EXHIBITION, 1900.—The following extract is from the Report of His Majesty's Commissioners for the Paris International Exhibition, 1900:—

"Scientific and Pictorial Photography. *Section A. Technical & Scientific Photography.* The most important exhibit in Section A is a beautiful collection of stellar transparencies by the Royal Observatory, Greenwich. Although this exhibit is a small one, the jury awarded it a Grand Prix, placing it third on the list in Class 12 in order of merit."

The exhibit placed first on the list was that of Messrs. Lumière and Sons of Lyons, which consisted of photographic plates, papers, &c. The second was that of the Imperial School of Graphic Art at Vienna (Dr. Eder, President), who exhibited specimens of photographic processes.

Other notable exhibits in the Section were those of Sir Norman Lockyer (spectrum and astronomical photographs) and of Capt. E. H. Hills, R.E. (photographs of the eclipse of 1898).

The only Grand Prix awarded to an optician in Class 12 fell to Messrs Zeiss, of Jena. To avoid any misunderstanding, the jury adopted the following resolution and directed that it should be inserted in their official report:—

"In placing the house of Messrs. Carl Zeiss in the first rank among all the opticians of France, Germany, and England, the International Jury has desired expressly and exclusively to render homage to the initiative taken in the recent transformation of photographic optics by the eminent scientific men who direct this establisment."

Grand Prix were also awarded to the Astronomer Royal in Class 15 (Mathematical and Scientific Instruments), and to the Royal Observatory, Greenwich, to the Ordnance Survey Department, to the Geological Survey Department, and to the Meteorological Council in Class 14 (Maps, &c.).

THE BRITISH ANTARCTIC EXPEDITION.—There appears to have been some bad-feeling in the matter of the arrangements for the voyage of the ship 'Discovery,' which has started, under the auspices of a Joint Committee of the Royal and Geographical Societies, on a scientific expedition to the South Polar Regions. The result of this is that Prof. Gregory resigned his position as Scientific Director, and Dr. George Murray has been appointed in his place; but this gentleman's journey is to be limited by Australia or New Zealand. It is well sometimes to see things from another's point of view, and on this principle the following extract is made from a leading American scientific journal:—

> The resignation of Professor J. W. Gregory from the scientific staff of the British Antarctic Expedition is unfortunate, both because he possessed peculiar qualifications for his post, and because it has brought to light dissensions among those interested in the success of the expedition. The question at issue between the Royal Geographical Society, on the one hand, and the Royal Society, or some of its members, on the other, is one frequently recurring, namely, should the executive command of scientific work be entrusted to a scientific man or is this unnecessary? When it was first arranged that Professor Gregory should take part in the expedition, it was understood that he would be the scientific leader. The British Government, however, gave a liberal subsidy, and a naval officer, Lieutenant Robert F. Scott, was appointed commander, Professor Gregory being made head of the civilian scientific staff. The relative position of Captain Scott and Professor Gregory gave rise to friction, Sir Clements Markham and the Royal Geographical Society holding that the scientific work was under the control of the naval officer in command. There were numerous conferences, and Professor Gregory finally consented to be satisfied with the control of a party to be landed on the coast. When, however, it was decided that the party should only be landed if this did not

interfere with geographical exploration, Professor Gregory resigned. It seems evident that a scientific expedition can have but one leader, and it is natural that the Royal Geographical Society should regard exploration rather than geological and biological research as the primary object in the present case. The results will depend on the personality of Captain Scott, an unknown quantity in America; he may simply engage in adventure, or he may prove himself a competent scientific leader. The German expedition, with Dr. von Drygalski in absolute control, has, however, an advantage from the scientific point of view.

THE *Popular Science Monthly*, from which the above is quoted, is a high-class American magazine of long standing, which deserves to be better known in England than it is. We have already mentioned the valuable series of papers contributed to it by Prof. Newcomb under the title "Chapters on the Stars," and there have been others of a like nature. But besides, the magazine contains articles on science of all kinds, written accurately, but in a popular vein. Among other things in the July number will be found an article on "Transmission of Yellow Fever by Mosquitoes." Another shows how a former land connection between America and Asia is proved by the snails along the coast. Prof. Thorndike describes recent experiments with monkeys. This will show the wide scope of the publication.

THE WEATHER IN JULY.—Until the 22nd the weather was very bright and sunny, with an undue preponderance of north-easterly and easterly winds. For 20 days an absolute drought prevailed, and the temperature exceeded 80° on 10 days. The highest recorded temperature was 87°·9 on the 19th, the mean temperature for the 22 days (July 1–22) exceeding the average by 3°·6.

A change to cold, thundery, and showery weather occurred on the 23rd, the rainfall in the next six days exceeding an inch and a half. The thunderstorm on the 25th was very severe in some parts of London, and the rainfall in West London was excessive, whilst at Greenwich the fall was remarkably small. There was a return to warmer weather after the 29th. W. C. N.

VERY DEEP-SUNK THERMOMETERS.—The notice of the Radcliffe deep-sunk thermometers on p. 316 reminds us of the fact, possibly not known to all our readers, that the longest thermometer in England, possibly in the world, is at Harestock near Winchester, at the Observatory of Colonel Knight, who has a series of deep-sunk thermometers, the bulb of the longest being at a depth of 70 feet. Colonel Knight is much interested in the secular change of temperature of the earth, and, thinking that this is best measured by earth-thermometers, believes that his records will be a valuable legacy to posterity.

It is stated, in Loomis's 'Treatise on Meteorology,' that a thermometer has been kept for 75 years in the vaults of the Paris Observatory, at a depth of 91 feet below the surface, whose reading has not varied half a degree during that period; but this does not come within the same category as that of Colonel Knight.

A NEW magnetic observatory is being established in France in the small village of Villepreux, thirty miles from the Parc St. Maur, the observatory at that place having become useless owing to the electric railways in the neighbourhood, which do not insulate their returns. The railway companies have had to pay a sum of about £1200 to the Government to pay for the expense of the new establishment, which will be under the charge of M. Moureau, who will continue to live at St. Maur.

FROM AN OXFORD NOTE-BOOK.

PERHAPS the greatest surprise of the last Visitation Day at Greenwich was to find ourselves travelling on the proper side of the railway-lines. All the years I can remember we have travelled to and from Greenwich on the *right* of the rails, instead of, as usual, on the left; and we always believed that this was a necessity which arose in some mysterious way from the crossing at Cannon Street. It seemed certain that there must be some grave reason, for the existence of such a curious exception to the ordinary rule was not without dangers, and it was assumed that such risks were not lightly incurred. But apparently a way out of the difficulty has been found, and the new Century has commenced with at least one reform of a noteworthy character. I have heard it said that some features of the railway time-table for the Greenwich service were due to Airy's suggestion, but cannot find any foundation for the statement, either in the "Autobiography" or by enquiry. Airy had a good deal to do with railways, especially in settling the proper gauge, and in keeping them away from the neighbourhood of the Observatory; but up to the present I have not been able to trace his influence on the working of the South-Eastern line. It may be remarked that there is at least one other astronomer who has interested himself in railway topics. Prof. C. V. L. Charlier, Director of the Lund Observatory, has gone into the question of computing equitable railway fares theoretically *, starting from statistics as to the number of travellers for short and for long journeys; with what success I fear I cannot say, but there are some interesting-looking curves and integrals in the 40 pages of his pamphlet.

THE following extract from *The Standard* seems worthy of a wider circulation:—

---

* Grundzüge einer Theorie zur Berechnung von Eisenbahntarifen. (Archiv für Eisenbahnwesen. Julius Springer, Berlin.)

A BRILLIANT METEOR.

TO THE EDITOR OF THE STANDARD.

SIR,—May I ask, through the medium of *The Standard*, if any of your readers noticed a very brilliant meteoric body in the North-East last night, between half-past eleven and half-past twelve p.m., moving rapidly in an Easterly direction, sometimes with a semicircular motion, occasionally completing a circle, rising and falling in a remarkable manner, being visible to the naked eye the whole of the time? Its varying colours were beautiful to the extreme.

About ten minutes to twelve another heavenly body was observed to shoot across its path.

I am, Sir, your obedient servant,
WALTER J. NEILSON.

48 Orford Road, Walthamstow, Essex,
June 7, 1901.

WE have seen several attempts lately to reduce the labour of computing "star-corrections"—Mr. Finlay has published some tables; those of Mr. Stone have been revised; and I believe Mr. P. H. Cowell has recently got another system in working order at Greenwich. The essential point is that of adapting the expression

$$Aa + Bb + Cc + Dd$$

to logarithmic computation, and all those who have tried it are familiar with the difficulties. Two of these terms can be combined, but not all four; and even by the combination of pairs little is gained. The fact is, such expressions do *not* lend themselves readily to logarithmic computation, and they *do* lend themselves to mechanical methods. Almost any form of arithmometer does such work well, and the time has come to introduce the arithmometer into the observatory. Such expressions occur in other ways besides star-corrections—in the linear tranformations for reducing stellar photographs, for instance (such as $ax + by + c$), or more generally in any transformation involving direction-cosines ($l\lambda + m\mu + n\nu$). In fact, directly we leave plane geometry to come to solid (even for the geometry of a surface like that of the sphere), we find expressions of this form for which the arithmometer is suitable and the logarithmic methods are not. Mr. S. A. Saunder has proved by practical experience how valuable the arithmometer is for such transformations in his work on the Moon. In glancing through an excellent account of the comptometer the other day by Mr. C. V. Boys (*Nature*, July 11, 1901), I was delighted to come across the following passage, which puts the matter so clearly that it is worth reproducing *verbatim* :—

> The comptometer is like all arithmometers in that, having found one product of two or more numbers, or having any previous result on the register, any further products of two numbers may be added to or subtracted from this, one at a time, without the necessity of writing down any intermediate result or of separately finding these products . . . . . The operation therefore that these machines can perform with the greatest advantage is of the form
>
> $$\frac{ab \pm cd \pm ef \pm \ldots\ldots}{r}$$

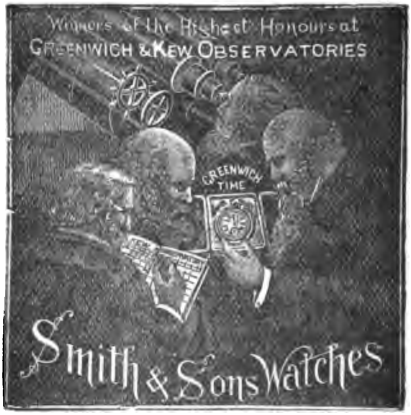
whereas the operation that is most favourable for the use of logarithms is of the form

$$\frac{a^n b^m \,\mathrm{tab}^p\, \theta}{r^q s^t \,\mathrm{tab}^{p'}\, \phi},$$

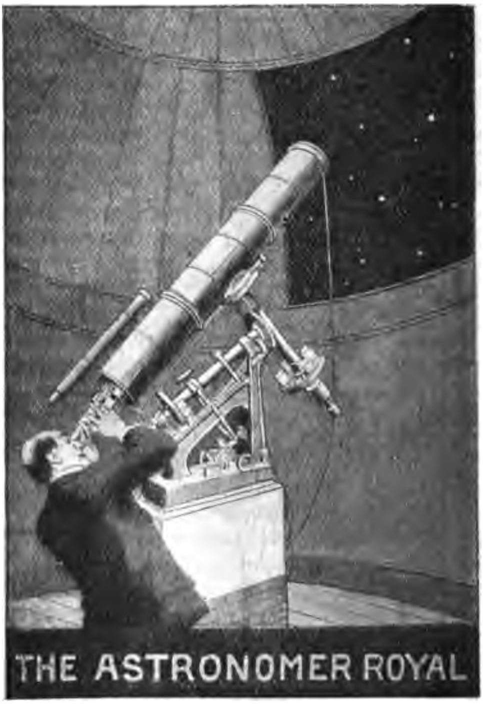
tab representing any of the tabulated logarithmic (? trigonometrical) functions. This advantage is so great that formulæ are artificially manipulated until they are finally rammed into this form and are then said to be adapted to logarithmic computation. Now the advantages of the calculating machines referred to are so great and they are in so many ways preferable to logarithms where they can be used, that it is just as important to adapt formulæ to mechanical computation by putting them where convenient into the first of these two forms. Then according as they can be put into one or other of these forms, machines or logarithms should be used for the purpose of computation, and no attempt should be made to use either for work specially adapted in this way for the other.

It may perhaps be worth while, by way of example, to mention that in the large number of corrections of the scale readings to bring them to circular measure that I had to make in my experiments on the constant of gravitation I found I could calculate $\theta - \frac{1}{3}\theta^3 + \frac{1}{5}\theta^5$ in less time on an arithmometer than was required to look up the angle in the trigometrical tables.

THE Editors of this Magazine have not usually been called upon for illustrations of these 'Notes,' though on one occasion they made an exception themselves. Perhaps they will allow of two pictorial advertisements being reproduced in the text, which may amuse others as they have me. In the first, four astronomers of the Kew Observatory, with long white beards, are using an equatorial (I cannot think the eye end shown belongs to anything but an equatorial) to find the Greenwich time. Messrs. Smith & Sons have kindly given permission for a permanent record to be made of this observation.


Winners of the Highest Honours at
Greenwich & Kew Observatories
Greenwich Time
Smith & Sons Watches

The second is quite modern and is also an extract from an advertisement, which is in its entirety rather too big for reproduction, occupying a full page of *The Graphic*; but the rest of it is not of great importance. The Astronomer Royal is regarding Mars and getting the signal for Bird's Custard Powder from it.



They have removed the venerable beard which is usually deemed necessary for an astronomer, but they have removed nearly all the other hair also. As regards other details, perhaps I may quote from the correspondent who kindly sent the cutting:—

> Have you heard of this catastrophe? The latitude of Greenwich has fallen to about 30° and Mars has become a close circumpolar. I can find no suggestions as to the cause unless it is THE COMET. What an anxious time they must have been having at Greenwich! No wonder the Astronomer Royal has grown bald! Possibly the larger instruments are not yet adapted to the new conditions, but I doubt whether we know all. The Astronomer Royal seems very much afraid that the one telescope he has got will run away from him. The new attachment of driving-gear too seems interesting.

SOME facts and figures courteously supplied by the Editor of the *Astrophysical Journal* in reply to my query (in these Notes for May last) make it clear that any failure to acknowledge the source of reprints is quite accidental and exceptional. There being, however, some little doubt as to the permission to quote the letter in print, I have referred to the writer and must await his reply before saying more.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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An Eclipse Expedition, 1901 *May.*

Early in February I received a letter one morning from Greenwich suggesting that I should form one of a proposed party of four to go to the Dutch East Indies with the instruments used by the Astronomer Royal in observing the eclipse last year at Ovar in Portugal, and intimating that a Man of War would probably be placed at our disposal for the forthcoming eclipse on May 18th. In a postcript it was suggested that tigers were believed to exist in the jungles of Sumatra in such numbers that there might be some difficulty in firing a rifle without hitting one.

Not mentioning the tigers, but putting special emphasis on the Man of War, in an airy manner I asked my family at breakfast what should be my course if the honour were really done me of asking me to accompany the Greenwich expedition in the capacity of an observer. To my great surprise the verdict was unanimous, that I must go if I could possibly arrange my affairs so that I could be away from home during the time necessary for such a long trip. Having thus secured my wife's leave, I promptly wrote off, if the Admiralty provided a vessel, I would go; and on the 21st of February I heard from Mr. Dyson that a ship would be given us, and that the party would consist of Mr. Turner from Oxford, Mr. Newall from Cambridge, Mr. Dyson from Greenwich, and myself. It was finally arranged that we were to proceed by the Nederlands Line boat 'Kœningin Regentes' from Genoa, straight to Padang in Sumatra, where Mr. Dyson and myself were to be picked up by H.M.S. 'Pomone,' and with the Greenwich instruments in our care were to be thus conveyed to the most likely spot on the line of totality.

On Friday the 8th of March, after a most pleasant meeting of the Royal Astronomical Society, and dinner afterwards, I took my final orders from the Astronomer Royal near Charing Cross railway station at 12 o'clock at night: these were, that as we should travel on board the same liner that the Dutch astronomers were going out upon, and as very great preparations had been made for their accommodation and no expense spared, by the Dutch Colonial Government on their expedition, we should endeavour on our way out, and immediately after our arrival, to find from them where we had best establish our instruments, and form no fixed plan of where we should go until we had exhausted every source of information on the spot and from the best authorities.

I left Charing Cross on Sunday night the 10th of March, intending to spend the following day in Paris and proceed to Genoa to join Mr. Dyson and go on board the Dutch liner on the 14th. After a most comfortable journey through Italy, I arrived at my destination at 2 o'clock on Wednesday, and found that in consequence of a storm in the Bay of Biscay the 'Kœnigin Regentes' had not turned up. I was rowed about for a couple of hours by an Italian boatman before I discovered this, and as I could speak his language no better than he could speak mine, and Ciceronian Latin was of no avail in searching for my ship, I was obliged to attempt to return to land; this was attended by no small degree of difficulty, as the Douan officials insisted on examining my luggage and charging me for my rifles and cartridges, under the impression that I was landing from some vessel in the Port. I had a terrible fight, from which I finally emerged victorious without paying a penny to the Imperial Customs, or to the very officious porters who had insisted on carrying my things to the Custom House instead of to the vehicle I had engaged to take me to an hotel. After this excitement, a bath and a good dinner were necessary to bring me to a proper frame of mind to meet my friends, and I was enjoying a postprandial cigar on my way to the post office when I heard my name shouted, and there in the thick of an Italian crowd I found Mr. Dyson and Mr. and Mrs. Newall, who had also dined and come out to see the fun, and walk about this very interesting city. I had learned that four American astronomers were in Genoa, and we hunted them up, hoping to find some one we knew among them. They proved to be the party sent out by the Massachusetts Institute of Technology, and under the leadership of Professor Burton. They were going by the same ship as ourselves, and their subsequent friendship added greatly to the pleasure of our trip. We saw as much of the sights of Genoa as we could on the rainy morning of March the 14th, and then at 2 o'clock went on board the 'Kœnigin Regentes.' Each of the Dutch passengers came up in turn and made himself known to us with the politest possible bow; thus the whole ship's company were at once placed on a most agreeable footing, which continued during the entire voyage. The stewards and cooks were all Malay, and very well they served us; in fact a restaurant in London run with Java boys as waiters would be the most paying thing I can think of, and a place where I should strongly advise the Royal Astronomical Club to dine. No matches were allowed on board, but when anyone wanted a light, if no cheery Malay had anticipated your wishes, you called out "Arpee" and some one sprung to your side with a burning stick of scented incense. I shall miss my Malay boys all the rest of my life. We found at dinner-time that at the Captain's table, besides two daughters of the Governor of the Dutch East Indies, were the four members of the Dutch astronomical expedition, the four American astronomers, Mr. and Mrs. Newall, and ourselves, making up with a General and a Judge a very happy and friendly party.

The first day of our voyage turned out to be wet and uncomfortable, but this was our only experience of the sort, and on Saturday with land in sight all the day and Stromboli throwing out rocks on the one hand and Etna smoke from its snow-capped peak on the other, I never remember a more beautiful view. Except for a block in the Suez canal, when a Kamseen was blowing and the thermometer stood at 99° in the shade, the whole trip was most enjoyable, and we were sorry to part with our excellent captain and ship's company at 6 o'clock on Saturday the 6th of April, when we arrived in the lovely harbour of Emma Haven in Sumatra. To our dismay we found that the American Government Expedition were in front of us, and the Stars and Stripes were flying on a small transport, the 'General Alava,' once a Spanish gunboat. We had the pleasure of making the acquaintance of Professor Skinner and all the American astronomers afterwards, and very kind friends they proved to be, giving all of us every assistance in their power. I had boasted before I started of American hospitality to Britishers; Mr. Dyson sampled it, and would now "go one better" if he could. The Dutch, under Major Müller, received us magnificently and ran us up to Pádang with a special train and saloon car, and we had our first experience of white duck suits, and a "rice table" at the Oranje Hotel. I am sorry to say I put my foot into it the first thing, by making a swimming-bath of a douche, and was told at dinner I might have got into hot water over it if it were known to the natives. On Monday, April 8th, we English astronomers called upon the Governor of Sumatra and were most kindly received. On seeing the mountains of this coast I had formed a very strong opinion as to the advantage of getting as far away from them as we could with our instruments, believing that they must attract more than their share of clouds, in this very rainy country. I therefore was anxious to take advantage of our having a Man of War, and steam away to an island about 70 miles west of Padang on the central line of totality. Governor Joekes warned us against the savage inhabitants, and seemed so anxious that we should not go there, that Mr. Dyson compromised matters by accepting a small coral island called Aoer Gadang, about 6 miles west of the Dutch camp, on the mainland. We arranged to go in the Governor's yacht the next day with the Malay chief who owned the island, to visit it and select a site. On the evening of this day we heard that

H.M.S. 'Pigmy,' instead of the 'Pomone,' had arrived in the harbour. I was disappointed, as there was no ice-machine on the former, and as all our correspondence was addressed to the latter I only received about three letters and papers during five months, and as I refused the Captain's kind offer of his cabin I had to requisition the bridge of the Man of War to "sling my cot." We had also to send the 'Pigmy' about 30 miles each week for fresh provisions and ice for our developing-room. During the absence of our ship Mr. Dyson and myself were marooned upon our desert island from the Friday night to the Monday morning. However the anchorage off Aoer Gadang proved all that could be wished, and I look back to my 50 days on board the 'Pigmy' as one of my most pleasant memories.

I have not space to describe the marvellously clever buildings erected for our instruments and ourselves by the Malays, solely out of bamboo and palm-leaf, but they effectually withstood the tropical sun and rain, and we neither of us were sick or sorry a single day in our most picturesque camp, our only regret being that we could not transfer all the buildings, as they stood, to England.

I found that the best assistance I could give to Mr. Dyson was to leave the whole plan of the erection and installation of the instruments to him, only acting as his Clerk of the Works, using my chief endeavours to allay his anxiety, and prevent him from overworking himself. I had hoped to get a few days of sport before the all-important 18th May, but I found that one Sunday off, when we were both invited to shoot wild boar, before our real work began, was all that I dare manage, and I take some credit to myself that Mr. Dyson did not wear himself out till the eventful day was past. We had only one neighbour on our island, an outcast heathen of eighty years, who kept the most appalling ape that ever I did see. This monkey was used by his owner for the purpose of climbing the gigantic cocoanut palms and throwing down the fruit, which he did in the most artistic manner by screwing the nuts off with his powerful arms, while he hung by his legs 70 to 100 feet from the ground. Mr. Dyson was especially warned not to go too near this animal, lest it should mistake his head for a cocoanut, when the first twist would be certain death after it had once made good its hold. I became most attached to this old Malay pirate, and with the help of a vocabulary, we held long conversations. He was the most perfect gentleman in his ideas, and never accepted even an empty soda-water bottle without making us some return, in the way of a present. He evinced the greatest interest in astronomy, and to see him, with hardly a rag on his body, explaining to casual strangers, who called, the merits of the different telescopes, and how they were "going to be used to wipe out the Sun," was most comical. A sparklet had a most exhilarating effect on him, and he considered it much too precious to be wasted on a follower of Mahomet, to whom he said spirits are forbidden, and who are consequently to be despised by a free-thinking cannibal. Our leave-taking was most touching, he giving me a present of rice, which I had to sprinkle on my bare head.

On the 18th of May I had been up each hour after 12 o'clock to see the most inky clouds over the whole surface of the sky, and no prospect of any improvement—so much so that when Mr. Dyson came to my berth at 6 o'clock I refused to be comforted, and turned my face to the wall, intending never to go near the instruments, except to pack them up. However, at 8 o'clock the Captain said "We are going to have a fine day after all"; and now the fun began. We rushed ashore, and it was as good as the Liverpool Steeplechase to see the clouds race across the ground-glass of the big camera, as the moment of the eclipse arrived. Our whole complement were in their places, and the search-light displayed for the benefit of the Dutchmen on the mainland—when we "doused our glim" totality was complete, sixteen seconds before theirs began; this signal we gave them to a second, and they were grateful. I leave my colleague to tell what happened during the eclipse and the results we obtained. The anchor of the 'Pigmy' was up within an hour of the last contact, and I went off tiger-shooting, leaving Mr. Dyson with about as hard a week's work, in the developing-room, as he ever did, in a temperature never under 80°, quite expecting to find him dead on my return. He wasn't; he was never better in his life, but all those who helped him in my absence were in hospital. J. J. Atkinson.

After the Eclipse, 1901 *May.*

Notes of the Return Journey from Sumatra.

On Whit Monday, May the 27th, the observers of the Eclipse congregated from different parts of Sumatra at the Oranje Hotel, compared notes, and were as cheerful as could be expected under the circumstances. Most of us were leaving Sumatra the next day, and we said farewell in hopes of meeting again not later than at the 1905 Eclipse. When I explained that I was returning *via* America, Prof. Barnard, assisted by one or two of the Naval Observatory Astronomers, was good enough to spend an hour or more in planning out my route across the Continent, and to give me directions for travelling, besides kindly inviting me to stop at his house when I went to the Yerkes Observatory.

Early next morning the 'Pigmy' sailed for Singapore, going round the south of Sumatra, close by Krakatoa. Mr. Atkinson and I arrived at Singapore on June 2, and while waiting for a ship to Hong Kong were overtaken by Prof. Burton and the party from the Institute of Technology, who proposed to swing their pendulum for the determination of the force of gravity there. We also, at Singapore, met Prof. and Mrs. Todd, who had come from the other side ot Sumatra. The weather with them had not been favourable and they were consoling themselves by taking a trip to Burmah.

At Hong Kong we had only one day to wait for the 'Nippon Maru' to San Francisco. The 'Nippon' called first at Japan, where Mr. Atkinson left, and then at Honolulu. One of our travelling companions from Honolulu remembered the Transit of Venus and Col. Tupman's expedition. The 'Nippon' arrived at San Francisco on July 8.

On July 9 I went to San José, and took the stage-coach next morning to Mt. Hamilton. After a delightful drive of 26 miles up the mountain, the observatory was reached a little before one o'clock. Professor Campbell kindly took me round the Observatory and introduced me to his staff; he explained that he had just come back from a fortnight's holiday, and in his absence Mr. Wright had found two stars with variable motion in the line of sight. He hopes to send Mr. Wright to Arequipa for two years at least to make line of sight observations, and contemplates the possibility of a permanent annex to the Lick Observatory in the Southern Hemisphere. At night I was shown the 36-inch; the definition was excellent, and it was of interest as showing the actual size of star discs, that with a power of 2000 a double star of about the 9th magnitude whose separation was 0″·3 was shown with the discs 5 or 6 of their own diameters apart. The one drawback to life on the mountain seems to be occasional rattlesnakes in the hot weather; we came across one within a few yards of Prof. Campbell's front door, which he vigorously attacked with big stones and a piece of gas-pipe. Apart from this and the fact that some people find it difficult to sleep in the rare air, Mt. Hamilton seems an ideal place for an observatory, at any rate, in the summer.

At Chicago I was fortunate enough to meet Professor Burnham, who had just returned from spending the Saturday and Sunday observing at the Yerkes Observatory. He had slept about four hours in two days, and was intending to make up for the loss of sleep by a 25-mile bicycle ride. He was very decided that the corona ought to be examined visually as well as photographically. At Evanston, near Chicago, Prof. Hough showed me his observatory, and Professor Crew his laboratory, and Professor Crew and I had a look through Professor Hough's 18-inch at night, leaving him at work observing Jupiter, as the definition was pretty good. At the Chicago University Professor Moulton took me in hand, and afterwards, to save me any difficulty in finding my way, accompanied me 7 or 8 miles to the station, where he introduced me to Professor and Mrs. Frost and Mr. Ritchey, with whom I travelled to Yerkes Observatory, at Williams Bay, Lake Geneva. A beautiful spot has been chosen on which to build a beautiful observatory. The weather is not so uniformly good as at Lick, and we had to wait three nights for a view through the 40-inch. After the 40-inch the most interesting feature of the Yerkes Observatory is the workshop. A large coelostat with a 5-foot mirror and clock complete has been just finished, practically all in the observatory. Mr. Ritchey was engaged on a 3-foot convex. Professor Hale explained that he hoped to use these with a grating for spectra of bright stars, &c. Another point of great interest was Mr. Ritchey's photographs with the 40-inch through colour-screens, which are a great advance in lunar photography. Prof. Hale and Prof. Frost were expecting new prisms for their line-of-sight work, and wanting to get to work again with the spectroscope.

From Chicago I hurried off to Boston and had a few hours at Harvard. Professor Pickering showed me the photometers, the instruments with which the stars and their spectra are automatically obtained, and his shelves full of photographs. He has material in hand for 28 more volumes of the Harvard Annals. Among minor matters of interest was a very pretty photograph of the spectrum of lightning which had just been obtained.

Unfortunately my time was too short for a visit to Washington. To spend only a fortnight in America seems like a parody on the way some Americans are supposed to visit Europe. Still a fortnight was long enough for a delightful tour and to see several places and many people I shall not forget. F. W. Dyson.

The Spectrum of Nova Persei.

The spectrum of Nova Persei was sensibly that of an ordinary helium-star when it rose to maximum on February 23, 1901. There were barely discernible bright edges to some of the hydrogen lines; nothing more. It was not until twenty-four hours later that they developed into the coupled bright and dark series characteristic of Novæ. This subsequence was the most distinctively new feature of the apparition. We need not, indeed, suppose it to have been exceptionally present. Its novelty to us resulted from the unprecedented promptitude of the spectroscopic observations made on the last critical occasion. Its theoretical importance was, however, crucial, as regards the line-displacements manifested in the double spectrum. Two modes of explaining them on Doppler's principle were in vogue. The first and most obvious ascribed the bright and dark constituents to two stars, the encountering velocities of which were signified by the opposite shiftings of the emission- and absorption-spectra emanating from them respectively. But on this rationale, the arrangement, relatively to one another, of the contrasted sets of lines could only be fortuitous; and in nature it was evidently systematic. In the course of a decade, four temporary stars were observed to exhibit this peculiar type of spectrum, and in all, the dark components of its duplicated series were the more refrangible. Yet no reason was conceivable why stars with cool atmospheres should unanimously approach the Earth at tremendous rates of speed, while stars with hot atmospheres agreed in retiring from it. The incredibility was glaring; the theory collapsed beneath the accumulated weight of successive coincidences. The *coup de grâce* was given to it by the circumstance above referred to. For the light-storm, by which the supposed partial collision was signalled across space, had burst, and was subsiding, before any signs of relative motion became apparent. Velocities of translation, as Mr. W. H. Pickering pointed out *, were then certainly *not* represented by the line-shiftings in the spectrum of Nova Persei. This, at any rate, fixes a landmark in the discussion.

Velocities of explosion did not, however, appear to be excluded; and they might be due either to the disruption of one body or the impact of two. Under the former shape, the hypothesis was advocated by Mr. W. H. Pickering †; under the latter, it has been recently suggested by Lord Kelvin ‡. In either case, absorptive effects could only be produced by the gaseous masses rushing in our direction ; and those with the highest velocities, being subject to quicker cooling by expansion, would soonest reach the stage of temperature enabling them to arrest light. The less mobile hydrogen (to mention the substance chiefly in question) would, on the other hand, continue hot, and its radiations, because inappreciably displaced, would shine on the redward sides of the corresponding absorption-lines. But this view is open to serious objections. To begin with, the self-refrigeration of the approaching hydrogen could not be stayed at a given point. A few hours after the catastrophe it was already, *ex hypothesi*, cool enough to absorb light transmitted through it; in a few more hours it would most probably have been too cool to give out any light of its own. But only incandescent hydrogen exercises any perceptible action upon luminous vibrations. Below the glowing-point it is indifferent and inert. Its direct and its inverted spectrum are simultaneously effaced.

Again, the initial velocities of the expelled gases, even if sufficient to carry them finally away from the parent-body, should be steadily diminished by its attraction. Solar prominences exhaust the speed of their formative impulse in a few minutes ; and, after an interval not much longer, the materials expelled from Novæ would possess only whatever remnant of velocity might survive the subtraction from their initial movement of the critical, or gravitationally controllable, rate. These are not the only difficulties that hamper the eruptive theory of new stars ; others, equally formidable, might be alleged; but their enumeration is superfluous. Enough has been said to render intelligible the despair felt in many quarters of successfully applying the Doppler principle to the leading phenomena of temporary outbursts.

Recourse was then taken to the " pressure-shifts," detected in 1896 by Messrs. Humphreys and Mohler §, of the Johns Hopkins

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* 'Astrophysical Journal,' vol. xiii. p. 277.

† 'Astronomy and Astrophysics,' vol. xiii. p. 201.

‡ 'Observatory,' June 1901, p. 224.

§ 'Astrophysical Journal,' vol. iii. p. 114.

University. They found that augmentation of pressure in the enclosure round an electric arc occasioned a proportionate, though very slight, increase in the wave-lengths of the metallic rays given out by it. The line of experimentation thus struck out was pursued, under intensified conditions, by Dr. Wilsing of Potsdam*. Using a spark-discharge in water, he obtained pressures probably equivalent to several hundred atmospheres, with line-displacements of corresponding magnitudes. And not only were the lines displaced, but they were accompanied by dark satellites on their more refrangible sides. A colourable imitation of the Nova-spectrum was at last secured in the laboratory; yet with one essential difference. The shiftings were wholly towards the red, while in Novæ they are predominantly towards the blue. The contradiction was sought to be reconciled, first by Dr. Wilsing, and later, with more elaboration of contrivance, by Dr. Vogel †, through a supposed partial effacement of the absorption-rays where they met and mingled with their broad, bright companions; so that only their more refrangible sections remaining perceptible, an illusory effect of upward shifting was produced. It was not easy to see, however, how the absorption of a superincumbent layer could be masked by emissions from the hotter and denser gases placed beneath it, and, in fact, the dark lines in Nova Persei disclaimed that possibility by standing out, on Father Sidgreaves's plates of March 7 ‡, separated clearly, as a consequence of their notably shortened wave-lengths, from shining associates, remarkably distended, yet not far removed from their normal situations. Pressure-shifts do not, then, supply the key to the enigma.

A phenomenon so changeable as the spectrum of Nova Persei cannot, indeed, be rightly understood when it is viewed under one instantaneous aspect. Its time-relations have to be taken into account. Besides the permanent line-displacements, others were exemplified in it, apparently progressive or periodical. The behaviour of the D-lines, for instance, was in the highest degree perplexing. The group consisted of a dusky strip due to helium-absorption; of a broad, bright band produced by the coalescence of the sodium-pair; and of two fine lines superposed upon it, evidently by reversal. Now, in the early stages of the apparition their intersection of the bright band was appreciably central; but in the course of March it moved gradually away from them towards the violet, until, in April, the intersection was at the extreme lower margin of the shifted effusion §. Recurrent modifications of wave-length, on the other hand, were observed in the first member of the ultra-violet hydrogen series ($H\zeta$). During the term of the star's short-period variability, the line came out in photographs taken at minima higher up in the spectrum than at maxima, when its position was normal. This, it is true, may have been a deceptive effect. Father Sidgreaves thought that it resulted from the alternate emergence and obliteration of an ultra-violet cyanogen band [*]. However this be, there can be no doubt that the seeming displacement of $H\zeta$ is a trait common to the spectra of P Cygni, of β Lyræ, and of Nova Persei [†].

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* Sitzungsberichte der kön. Akad. der Wiss. zu Berlin, May 4, 1899; 'Astrophysical Journal,' vol. x. p. 113.

† 'Sitzungsberichte,' Berlin, March 21, 1901.

‡ 'Monthly Notices,' vol. lxi. p. 388.

§ 'Yerkes Observatory Bulletins,' Nos. 16, 17.

In Nova Persei, as in Nova Aurigæ, "green" seems to have outshone "yellow" helium. Father Cortie observed on March 21 the glow of λ 6678, the fundamental of one of the "parhelium" sets, and its neighbour at λ 4922 was generally recorded as bright. Yet D_3, as we have said, was visible only by absorption on Professor Hale's orthochromatic plates. Early in April, however, the band representative of it diffused towards the violet, with accompanying loss of intensity; and on April 21 a bright line sensibly coincident with D_3 was determined by Sir Norman Lockyer's assistants at South Kensington. No coupled helium-rays were at any time unmistakably present. Hydrogen alone gave a conspicuous double spectrum.

Temporary stars are not isolated phenomena. The baffling peculiarities of their light are met with as well in some few objects of a more permanent character. Such are P Cygni, β Lyræ, η Carinæ, and, in a minor degree, Alcyone, the leader of the Pleiades. Plainly, the bright and dark spectra of the entire group originate under analogous conditions, and must admit of a similar explanation. It is not permissible to adopt for the duplicated series in Nova Persei a rationale that cannot be applied to that in P Cygni. A state of things transitory in one case is permanent in the other. Explosions and eruptions must then be dismissed as inadequate. The full requirements of the situation cannot be met by a catastrophic explanation, however skilfully adapted to fit them.

A. M. CLERKE.

Telegraphing during Eclipses.

WHILE on the subject of solar astronomy, I think I may as well tell how a well-known English astronomer, Mr. E. W. Maunder, of Greenwich Observatory, was the presumably innocent cause of a great deal of useless trouble and still more useless expense in connection with the observations out here. Mr. Maunder was sent by Government to observe the eclipse at Mauritius, but prior to departing for that island, he entered into a great scientific compact with Professor Todd whereby it was arranged that Professor Todd should endeavour to get a clear wire from Mauritius to Padang, and Mr. Maunder should wire direct to Padang the moment that the eclipse at Mauritius was completed, and warn his scientific confreres up here of any peculiarity of the phenomenon—if such existed—that seemed worthy of particularly careful observation.

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[*] 'Monthly Notices,' vol. lxi. p. 462.

[†] A. C. Maury, 'Harvard Annals,' vol. xxviii. p. 102.

Professor Todd arrived at Singapore and went to the Eastern Extension Telegraph Office and solicited the goodwill and co-operation of the Company. This Mr. Grigor Taylor proceeded to secure for the scientist. Headquarters at London were consulted by cable and they consented *in toto*. Then the Eastern Telegraph Co. had to be consulted and The Eastern and South African Co., that runs from Bombay to Mauritius. Then the ponderous and slow machinery of the Indian Government had to be set in motion to secure permission to hold a clear line at a given hour on May 18, and, after the most careful and mature deliberation, and the writing of files beyond computation, the required permission was eventually graciously accorded. Then the almost equally ponderous machinery of the Netherlands East Indies Government Telegraphs had to be wound up in order to get the necessary message through from here to Padang. Eventually, however, everything was made ready, the eclipse came on, the lines were cleared round the immense and irregular arc of a circle by which despatches have to be sent from Mauritius to Padang, the march of cabled commerce was halted to permit of the passage of science, the breathless world paused. Presently, the world had to breathe and move again, but meanwhile the eclipse had passed. Not a word had been heard from Mr. Maunder, not a signal had been wired from Mauritius to Padang. Something over a thousand words had been cabled in making arrangements. The cable company had franked them all. Furthermore—with the aid of two Governments, and thanks to the assistance of about a dozen directors, agents, and other high telegraph officials—it had cleared thousands of miles of cables at the busiest hour of the day, and had secured the co-operation of the Government of India; but to this moment no one in Padang has received a word from the observer in Mauritius. How it happened nobody knows. Probably, in the excitement of the moment, the scientist forgot to wire — was lost in the contemplation of the eclipse. Professor Todd, who had gone to work in a single-handed way for the benefit of the several Padang Expeditions,—his own were at Linggi and Singkep—has had his pains as his reward; and, plus that, he cherishes the consciousness of having striven to do good, failed in the attempt; and he also cherishes an unbounded admiration for the liberality and enterprise of all these Telegraph Administrations who responded so promptly to his appeal in the cause of astronomical science.

We print above in full some paragraphs which appeared in the *Straits Times* for 1901 June 1. It does not seem to be a strained inference that these paragraphs were to some extent inspired by Prof. Todd; and we will venture to take them (subject to correction) as representing essentially a grievance stated by him. They have been given publicity in one sense; but the majority of astronomers naturally will not have seen them, and we think that such a statement ought to be made, and replied to, before the astronomical world; for there are important interests at stake.

The reply can only be given by Mr. Maunder himself. We do not know anything of the circumstances under which he entered into the compact, or why he failed to carry it out. It is probably now well known that he had the misfortune to fall seriously ill; and this has delayed his return to England, so that it is doubtful whether he can formulate his reply until next month.

Meanwhile we venture to offer some remarks on the general question, which we trust do not prejudice his case in any way: they might have been made equally well if he had telegraphed 1000 words.

The *Straits Times* upbraids Mr. Maunder because he did not send a telegram from Mauritius during the eclipse to the observers at Sumatra. The paper says that he "entered into a great scientific compact" with Prof. D. P. Todd to do so, and that Prof. Todd had single-handed and with great energy induced two Government departments and several telegraph companies to give a clear line at the time of the eclipse for this purpose. But we ask, What did he expect Mr. Maunder would have to telegraph that was worth all this trouble? It seems to us that it is time that this "telegraphing during eclipses," which looks very nice and scientific and enterprising in a newspaper paragraph, should stop. Suppose that Mr. Maunder had seen, or had thought he had seen, a comet near the Sun (we say "thought he had seen," because there is so much to do during the brief period of totality that an observer would not have time to make a careful examination), we feel sure that he would not have made such a definite pronouncement as a telegram entails, until he had seen it on his developed plate. If he had telegraphed that a comet was there, knowing the facility with which everyone can see what they are told to see, there would have been a chance that this object, which perhaps only existed in a Mauritius observer's mental eye, would have been carried down to posterity with an overwhelming weight of corroborative evidence from Sumatra. It has been suggested to us that a timely telegram might have given the Eastern observers information by which they could have better regulated their exposures; but surely the well-known remark about "Swapping horses" applies as strongly in an eclipse as anywhere. No definite code, about the strength of the coronal line, say, appears to have been made between the observers at Mauritius and Sumatra, and surely no one at the latter place was so vague in his programme that he would have altered anything at the last moment without a very definite reason. We hope we are not labouring this point, but this seems to be one of the kind of things that give astronomers a bad reputation as sensible people and we are glad to make a little protest. We may recall a remark lately made by a high-class American astronomer. The conversation was about the dependence of one science on another. Some one said what a help photography was to astronomy. "Oh, yes!" said the astronomer, "but how about electricity? Astronomers would be much more respected if the electric telegraph had never been invented." The obscurity of this remark is a little lightened when it is recalled that we have had telegraphic news of a ninth satellite of Saturn of which no more has been heard; that a new rotation period of Venus is received through the Kiel agency about once a year, with little evidence, but much importance, as though such a thing had never been before heard of; that quite recently the place of a comet 30° north of the Sun which did not exist was cabled; and it is not hard to remember other items of astronomical information, sent in a hurry, apparently to give them a fictitious interest, which would have been better kept until they could have been accompanied by full evidence. It may be thought on consideration that even in the case of Nova Persei and the recent Great Comet the help of the telegraph was scarcely necessary. The Nova was obvious to everyone, and the effect in England of the comet telegrams was only to keep several persons needlessly out of their beds, and to make one worthy gentleman think he had seen a comet which he hadn't. But we must not press this view of the case too far, for there are certainly occasions when a telegram is valuable.

EDITORS.

THE above was in print before Mr. Maunder's return, but it was brought to his notice as soon as possible, and we have no reason for altering any portion of it. The following is sufficient to clear Mr. Maunder from any blame in the matter:—

My reply to Prof. Todd is of the simplest. His instructions where and what to telegraph did not reach me until the afternoon of Thursday, May 23, five days after the eclipse. Until that date I supposed that he had abandoned the scheme, since he had not communicated either with the Eastern Telegraph officials at Aden or with those in Mauritius. I was unable to communicate with him as he had not furnished me with an address. He, of course, knew my address perfectly. E. WALTER MAUNDER.

The Orbits of Jupiter's Satellites.

IN the July and August numbers of the *Observatory* Prof. Sampson has given a long review of my discussion of the Cape heliometer observations of Jupiter's satellites. I am much indebted to him for this proof of interest in my work. In the course of this review Prof. Sampson calls attention to two points, on which he asks me "to throw some light, and to show that I have not ignored them." I am glad Prof. Sampson has found no more serious objections to my work than these two, both of which doubtlessly arise from some mistake on his part. As a matter of fact, neither of the two points was ignored by me, and both of them even now appear to me to need very little explanation in addition to that given in my paper.

The first point is about the mean distances. Prof. Sampson writes (*Observatory*, August 1901, p. 305):—"But Mr. de Sitter says nothing about Marth's treatment of the mean distances." But on page 15 of my paper I say:—"Marth's data and tables were very carefully examined and compared with Souillart's theory"; and then I enumerate the errors found. After this statement I did not think it necessary to refer expressly to every one of Marth's figures, which had been found correct. To Prof. Sampson, however, this is evidently not a sufficient guarantee for the correctness of the mean distances adopted by Marth and by me. On page 303 (*Observatory*, August) he gives Marth's values reduced to seconds of arc, and then asks the following questions:—"Whence did Marth derive these numbers, and what authority is there for associating them with any particular value of the mass of Jupiter?" to which the answer is: Marth computed them from the adopted mean motions by means of Kepler's third law, as will be explained below. And "What diameter of Jupiter and what ellipticity do they imply?" Answer: The diameter and ellipticity need not be known separately, but only the coefficient Jb^2, or, in Prof. Sampson's notation, $A^2(\rho-\frac{1}{2}\phi)$, the adopted value of which is stated on page 26 of my paper. Prof. Sampson then goes on to say:—"It is, at any rate, certain that they agree with none of the three sets recorded above" (those of Bessel, Schur, and Damoiseau; I will return to this point); "and whatever their source may be it is clear, and it is very important to recognize, that their association with one mass of Jupiter rather than another has no authority beyond the authority of this source, which is unknown, and is at present simply Marth's *ipse dixit*." And again (p. 305):—"I should be very much surprised if any one of his four mean distances can be shown to be correctly dependent upon the attached mass; to assume such to be the case for all four is an act of faith I am not equal to." Most certainly it is not desirable to have recourse to faith in a scientific question, and happily such is not necessary in the present case, as Marth's mean distances, far from having no other authority than his *ipse dixit*, can be verified by a very simple computation.

Kepler's third law, corrected for the effect of the compression of Jupiter, gives the formula

$$(1) \quad . \quad . \quad . \quad . \quad . \quad n_i^2 a_i^3 = f\frac{1+m_i}{M(1+\Sigma m)}\left(1+\frac{Jb^2}{a_i^2}\right),$$

where

n_i = mean motion of satellite.
a_i = mean distance „
m_i = mass „
M = mass of Sun.
J = $\rho-\frac{1}{2}\phi$ = coefficient depending on Jupiter's compression.
b = equatorial radius of Jupiter.
f = the Gaussian attraction-constant ($\log f = 6{\cdot}4711629 - 10$).

In this formula the unit of length is the mean distance of the Earth from the Sun, and the unit of mass is the mass of Jupiter's system. Allowing for the difference of notation, this formula is identical to that given by Prof. Sampson (*Obs.* p. 302), if the perturbative terms on the right are neglected.

If we adopt for the numerical computation the masses of Souillart, the mean motions of Marth or of Souillart (which comes to the same thing, for Marth's "plausible corrections" are negligible, and the largest of them only amounts to 0″·5 in 100 years), if further we take the values of Jb^2 and of a_i expressed in b as unit from Souillart (the latter from page 10 of his second memoir, quoted by Marth, *M. N.* vol. li. p. 516), and if we put

$$M = 1047{\cdot}700,$$

we get the following values of log a for the four satellites:—

7·450 1051, 7·651 7881, 7·854 5797, 8·099 7955.

Marth's values are:—

7·450 105, 7·651 788, 7·854 580, 8·099 795.

The adopted numerical values of all the elements are stated on page 26 of my paper. As the mean distances are there expressed in the semi-axis major of the Earth as unit, it is necessary to know the adopted value of b expressed in the same unit, which I regret to have omitted. It is

$$\log b = 6{\cdot}6684 - 10.$$

Prof. Sampson compares Marth's mean distances with those derived from observations by Bessel and Schur, and from an unknown source by Damoiseau.

This comparison is, however, not legitimate. Marth's distances were derived by the above formula (1) from the adopted value of the mass; Bessel's (and Schur's) distances, on the other hand, are each individually derived from observations, and from each of them Bessel derived a value of M by the formula (1), thus arriving at four independent determinations, viz.:—

1048·38, 1048·15, 1048·14, 1047·75,

of which the weighted mean is

1047·88.

It is thus not allowable to treat Bessel's four distances as corresponding to one and the same value of the mass.

Bessel observed the four satellites separately, and thus naturally reduced them separately, and finally adopted the mean of the four separate results. In the case of the Cape observations, however, the four satellites could not be separated, and one unknown was accordingly introduced instead of the four mean distances from the beginning. Prof. Sampson says (p. 305):—"It seems to me that the corrections to all the four mean distances should have figured in Mr. de Sitter's equations of condition, for by the assumption that underlies their elimination he suppresses the possibility of correcting them, and runs the risk, which is almost a certainty, of adding his correction to Jupiter's mass to a number which may have been incorrectly adopted by Marth." Now if Marth's mean distances had not been consistent with his adopted mass, evidently the best and the only course for me would have been to make them consistent, and then to proceed exactly as I did in my paper, viz., to eliminate them by expressing their corrections in terms of the correction to the mass. The only assumption which underlies this elimination is (see my paper, p. 18), that the mean motions are known with such accuracy that no real corrections to them could be derived from the Cape observations; and this assumption, I venture to think, Prof. Sampson will not object to.

I now come to the second point, viz., the fact that the quantity Jb^2, or $A^2(\rho-\frac{1}{2}\phi)$, was not introduced as an unknown. I must again refer Prof. Sampson to my paper, p. 20, where I say:—"The quantity J cannot be determined with any weight from these observations, and must therefore be taken as correct. The only means of determining it would be from secular perturbations in the nodes or perijovia."

The constant factor by which the mean distance is multiplied, owing to the compression of Jupiter, is

$$1+\frac{1}{3}\frac{Jb^2}{a^2(1+m)}.$$

Adopting the same numerical values as above, this factor becomes for the four satellites

1·000 225, 1·000 089, 1·000 033, 1·000 011.

When Jupiter is in opposition ($\log\Delta = 0{\cdot}6$), this corresponds to an increase of the apparent mean distances by the following quantities:

0″·033, 0″·021, 0″·012, 0″·007.

This is thus what I have called in my paper the *maximum geocentric effect* of this perturbation. As a general rule, I have only introduced as unknowns the corrections to those inequalities of which the maximum geocentric effect is above 0″·05, and as a matter of fact the three unknowns corresponding to inequalities just above this limit (viz., those that I called y_2, y_3, y_4) came out little larger or smaller than their probable errors. They might, indeed, as well have been neglected (see my paper, p. 70). It is not difficult *a posteriori* to estimate the probable error with which a correction to Jb^2 would have been determined by the Cape observations, if it had been introduced as an unknown. I find, by a rough computation, that this p. e. is more than one-third of the whole quantity to be corrected. It would be possible to determine the quantity J with even greater accuracy from the measured diameter and compression of Jupiter's disc and its observed rate of rotation, without referring to the motions of the satellites at all*.

W. DE SITTER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Anomalous Occultations.

GENTLEMEN,—

A peculiarly interesting case of non-instantaneous occultation may be added to those cited by Mr. Innes in your last Number. On January 6, 1876, during the passage of the Moon across the Pleiades, Hartwig, the Strassburg observer, noticed, or thought that he noticed, the immersion of Atlas to take place in two stages, a faint ray surviving, during six-tenths of a second, the extinction of the brilliant light (Winnecke, 'Astr. Nach.' No. 2074). Now Hartwig was unaware that this star was—so to speak—a lapsed double; his vision was unprepossessed. The companion, however, had been only once seen unequivocally. Struve, in 1827, found Atlas to be *duplex difficillima*, and entered it as Σ 483 in his great Catalogue. In 1830 the components could not be separated, but were thought to present a "wedged" appearance; while in 1836 the star was unmistakably single in a serene sky ('Mensuræ Micrometricæ,' p. 283). It has never since been divided, although Mr. Burnham has repeatedly submitted it to keen scrutiny ('Memoirs Roy. Astr. Society,' vol. xliv. p. 230).

Three possible explanations of the anomaly suggest themselves. Struve's original observation may have been illusory. Yet he himself saw no reason to distrust it; and it derived unexpected, though indirect, confirmation from the occultation-effect of 1876. Secondly, the companion might have disappeared, between 1827 and 1836, in consequence of its orbital motion. In this case, however, it should have long ago re-emerged to view. The third, and most probable, alternative is that Atlas is coupled with a variable satellite. Many combinations of the kind are suspected, if not ascertained, to exist; and, indeed, another member of the

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* I may be permitted to use this occasion to publish some of the more important typographical errors in my paper:—

Page 19, line 17, *for* $\frac{a_i''s_i}{\Delta'_i}$ *read* $\frac{a_i''\rho_i}{\Delta'_i}$.
" 20, " 10, *for* l_4 *read* e_4.
" 23, " 27, *for* $\frac{1}{1000}y_2 = 2\cdot4\,\Delta m_9$ *read* $\frac{1}{1000}y_3 = 2\cdot4\,\Delta m_4$.
" 26, " 7, *for* $\log a_1 = 7\cdot450\,150$ *read* $\log a_1 = 7\cdot450\,105$.
" 61, " 36, *for* $\mu = +0\cdot01583$ *read* $\mu = -0\cdot01583$.
" 64, *for* TABLE XVI. B *read* TABLE XVI. A.

Pleiades group has changed from obvious duplicity into apparent singleness within the last few years. This is a 7·2-magnitude star due south of Alcyone, catalogued by Wolf in 1876 as forming a wide pair with a ninth-magnitude companion. Nevertheless, it came out single in the Henry photograph of 1886, but conspicuously double in that of 1888. Then, once more, in 1895, the companion had vanished; M. Chevremont, at least, who drew attention to these curious facts, was unable to perceive it with a small refractor ('Bulletin de la Société Astronomique de France,' 1896, p. 295).

The enigma of the duplicity of Atlas may be solved by a recovery of brightness on the part of its obscured attendant, or by finding evidence of its gravitational influence in the variable radial velocity of the lustrous primary. This is one of many circumstances which lend special interest to spectroscopic motion-determinations of the Pleiades. Yours faithfully,

London, 1901, August 21. AGNES M. CLERKE.

Old Irish Records of Comets and Eclipses.

GENTLEMEN,—

The Annals of the Kingdom of Ireland by the Four Masters (as they are commonly called, the principal one being Michael O'Clery, who is thought to have died at Louvain in 1643) were published in Irish and English on opposite pages by Dr. O'Donovan in 1851. In his "Introductory Remarks" he gives a list (p. xlviii) of the astronomical phenomena which are to be found recorded in the 'Annals of Ulster,' a work compiled by Cahal Maguire, who died in 1498, and of which there are not only Bodleian and Dublin copies (in Latin), but an old English translation, evidently made from the Bodleian manuscript and now in the British Museum. The astronomical phenomena consist of eclipses and comets. Of the latter there are four: (1) in A.D. 614 *, no nearer date is recorded; (2) in A.D. 677, stated to be in September and October; (3) in A.D. 1018, "for 14 days in harvest;" and (4) in A.D. 1066, which in all probability (though the year seems here a little doubtful) relates to the appearance of Halley's Comet in the year of the Norman Conquest in England.

The comet of A.D. 614 is mentioned in the Byzantine records as having appeared at the time when Jerusalem was taken by Chosroes II., king of Persia, which would be about the month of June in that year. As regards the comet ascribed to A.D. 677, it would seem that instead of altering the date to a year later, as O'Donovan suggests, we should make it a year earlier; at least if

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* O'Donovan remarks that up to A.D. 1014 the dates as set down in the 'Annals' are one year too early.

Pingré is right in identifying that mentioned in Beda and other European records with one observed in China in A.D. 676. Several historians speak of one seen in the northern heavens in the month of August A.D. 1018, and this is confirmed by the Chinese records.

Now as regards the eclipses: O'Donovan states that their dates are confirmed by their agreement with those in the catalogue contained in 'L'Art de vérifier les Dates.' In most cases, however, only the year of the eclipse is mentioned, and I will set them down in order, and compare the solar eclipses with the tracks assigned in Oppolzer's 'Canon der Finsternisse.' In the Irish records, as corrected by O'Donovan, we have:—

A.D. 496. "Solis defectio."—An annular eclipse occurred in Eastern Europe and Western Asia (partial in the British Islands) on October 22 in that year.

A.D. 512. "Defectus solis contigit."—A total eclipse passed from the Mediterranean Sea into Central Asia on June 29, and was partially visible in the British Islands.

A.D. 591. "Defectus solis."—An annular eclipse crossed Africa on September 23, and might perhaps have been seen as a small partial one in Ireland some time before noon.

A.D. 664. "Tenebræ in Kalendis Maii in II horâ."—This eclipse (on May 1) is mentioned in the Anglo-Saxon Chronicle, where the date is erroneously given as May 11. The line of totality passed across the British Islands, including the north of Ireland.

A.D. 753. "Sol tenebrosus."—There were two annular eclipses this year, on Jan. 9 and Dec. 29; the former must be the one meant (if the note really refers to an eclipse), as it was central in Southern Europe and would be partially visible in the British Islands.

A.D. 763. "Sol tenebrosus in horâ tertiâ."—It does not appear that any eclipse could have been visible in Ireland this year, and the appearance must have been meteorological.

A.D. 865. "Eclipsis Solis in Kal. Januarii, et Eclipsis Lunæ in eodem anno."—A total eclipse of the Sun passed over the British Islands on the 1st January in that year, and a total one of the Moon took place on the 15th of the same month.

A.D. 885. "Eclipsis Solis et visæ sunt stellæ in Cœlo."—This is mentioned in the Anglo-Saxon Chronicle, merely with the remark that "the Sun was eclipsed." It occurred on the 16th of June, and was total in Northern Europe, including the extreme north of Great Britain.

A.D. 1023. "An Eclipse of the Moone on the 4th Id. of January, being Thursday. An eclipse of the Sunn the 27th of the same Moone, on Thursday."—A partial eclipse of the Moon did occur on the 9th of January, and a total one of the Sun on the 24th, so that there seems to be some mistake in the above dates; Jan. 24 *was* a Thursday, but Jan. 9 fell on a Wednesday.

A.D. 1031. "An Eclipse on the day before the Calends of

September."—Here there seems to be a mistake of a year, as a large eclipse of the Sun (total or annular in Scandinavia, and partial in the British Islands) did take place on the 31st of August in A.D. 1030, respecting which much has been written, attempting to connect it with the battle of Stiklastad, which, however (as is pointed out by Dr. Dreyer in the *Observatory*, vol. xviii. p. 363), was really fought on July 29, more than a month before the eclipse.

The above are all the solar eclipses collected by O'Donovan, but there are other lunar eclipses mentioned, as follows:—

A.D. 674. "Nubes tenuis et tremula ad speciem cœlestis arcus in vigilia noctis *vi feriâ ante pasca* ab oriente in occidentem per serenum cœlum apparuit. Luna in sanguinem versa est."—No eclipse of the Moon occurred that year, and the phenomenon was probably meteorological.

A.D. 692. "Luna in sanguineum colorem in Natali S. Martini versa est."—This also must have been meteorological.

A.D. 718. "Eclipsis lunæ in plenelunio."—Eclipses of the Moon *usually* occur at the time when she is Full; as the only lunar eclipse during the year in question was a small one on the 12th of November, that must be the one referred to, if this were really an eclipse.

A.D. 762. "Luna tenebrosa. Nox lucida in Autumno."—No eclipse of the Moon occurred that autumn, and the appearance was therefore meteorological.

A.D. 773. "Luna tenebrosa ii Nonas Decembris."—There was a total eclipse of the Moon on the 4th of that month in the morning.

A.D. 788. "Luna rubra in similitudinem sanguinis xii Kal. Martii."—There was a total eclipse of the Moon on the 26th of February in that year.

A.D. 807. "Luna in sanguinem versa est."—A total eclipse of the Moon occurred that year on the 26th of February in the morning, and a partial one on the 21st of August in the evening. An eclipse of the Sun is mentioned in the Anglo-Saxon Chronicle as occurring at 11 o'clock in the morning on the seventeenth day before the calends of August; but this is evidently a mistake for the calends of March, as an annular one did occur on the 11th of February in that year.

A.D. 878. "Eclipsis Lunæ Idibus Octobris iv lunæ."—A total eclipse of the Moon took place on the 15th of October in the morning. On the following 29th was the total eclipse of the Sun mentioned in the A.-S. Chronicle as falling in the year after Alfred's great battle with the Danes, which enables us, with great probability, to correct its date to A.D. 877, and that of the king's death to A.D. 899, so that the true millenary of the latter event is now past.

A.D. 921. "Eclipsis Lunæ xv. Kal. Jan. feriæ prima hora noctis."—A penumbral eclipse of the Moon occurred on the 28th of December, A.D. 920; but this can hardly be that referred to, so that the phenomenon was probably meteorological.

Yours faithfully,

Blackheath, 1901, Aug. 2. W. T. LYNN.

P.S.—Since writing the above, I have had an opportunity of consulting the Ulster Annals themselves. The expression which puzzled me with regard to the lunar eclipse of A.D. 877 [878], that it occurred "Idibus Octobris *iv Lunæ*," appears to be a misprint or mistake of O'Donovan, for the Annals state that it was on the 14*th* of the Moon, and that an eclipse of the Sun followed "on the 4th of the Kalends of November, the 28th of the Moon, about the 7th hour of the day"—this eclipse being the one also mentioned in the Anglo-Saxon Chronicle, the year after Alfred's victory over the Danes.

There is also a similar mistake under A.D. 663 [664], where the "tenebræ" said to have taken place "Kalendis Maii in II horâ" should be in the ninth hour, *i. e.* about three o'clock in the afternoon, when there was really a solar eclipse, referred to above.—W. T. L. (Aug. 13).

The late Professor Safford.

GENTLEMEN,—

The obituary notice of the late Professor Truman Henry Safford given in the August number of the *Observatory* makes no reference to his early years, when he showed himself to be one of the most remarkable calculating boys on record. At only ten years of age his extraordinary powers of calculation attracted so much attention that articles on the subject appeared in both American and English journals. Some accounts of this period of his life, for which I prepared a few notes 22 years ago, may not be uninteresting to your readers. The following information is derived from the account of the boy Safford published in *Chambers' Edinburgh Journal* for October 1847.

Truman Henry Safford was born at Royalton, Windsor County, Vermont, on January 6, 1836. His father was a farmer of considerable intelligence, from whom he inherited his mathematical tastes. The child was delicate and fragile and was reared with difficulty. A highly nervous temperament seems to have characterized his infancy and youth, which was much subdued in after life. His mental precocity seems to have been indicated so early as three years of age. At the age of six he was able to perform accurately, and entirely mentally, such a calculation as reducing 1040 rods to barleycorns.

Before his eighth year he had shown powers of mental calculation quite equal to those of the remarkable mathematical prodigy Zerah Colburn. It is remarked that these feats were not achieved by Safford by the kind of intuition usually observable in such cases, but by means of study, and it was observed that he improved rapidly by practice and lost proportionately when he neglected the cultivation of his powers. At this early age he acquired some knowledge of algebra and geometry, and in addition to the power of performing lengthy calculations in his head, he possessed the higher power of comprehending and solving abstruse and difficult questions in various branches of mathematics.

At the age of nine he studied Hutton's 'Mathematics,' and at this time Dr. Dewry, a well-known mathematician, wrote thus about him:—"He is not one of the calculators by instinct, but a real regular reasoner, on correct and established principles, taking the easiest and most direct course. As he had Hutton's Mathematics and wanted some logarithms he computed correctly the logarithms from 1 to 60 by the formula given by Hutton. He is a wonderful boy. His mind is too active, and when roused in the night, or made wakeful by his nervous temperament, it is often difficult to arrest the current of his thoughts on some interesting calculation."

At this time he constructed and published an Almanac which ran through two editions—one of 7000 copies and the second of 17,000—a remarkable achievement of a boy not 10 years old!

Numerous examples of his skill in solving arithmetical problems have been published, in performing which he evinced an intense degree of nervous excitement. When asked "What two numbers are those whose sum multiplied by the greater is equal to 77 and whose difference multiplied by the less is equal to 12?" he shot out of his chair like an arrow, flew about the room, his eyes wildly rolling in their sockets, and in about a minute gave the correct answer. In the mensuration of solids he displayed an extraordinary power. Being asked to give the solidity of the Earth the mean diameter being 7918·7 miles, he writhed about, flew rapidly about the room, flashed his eyes, and in about a minute said 259,992,792,083. Perhaps the most surprising effort of his mental power was evinced when he was asked to multiply in his head eighteen figures (365 repeated six times) by fifteen figures (365 repeated five times). In about a minute of highly excited condition he gave the answer of thirty-three figures correctly.

No wonder that a child of ten years of age with a mind so extraordinarily developed should have been considered predoomed to an early death; but as the remarkable faculty of Zerah Colburn disappeared as he grew to manhood, so did it happily result with Safford, and as he advanced in years, though a mathematician of no ordinary capacity, he seems to have grown out of the abnormal faculty which made his boyhood so remarkable.

Efforts were made to induce his father to take him round the country as a show, but it was more wisely decided to give him the benefit of a good education, and accordingly he was removed to

Harvard College, Cambridge (Mass.), where in the year 1846 he was placed under the charge of Professor Everett and Professor Pierce.

Yours faithfully,

32 Tavistock Square, W.C.
1901, Aug. 12.

E. B. KNOBEL.

Visibility of Jupiter's Satellites by the Naked Eye.

GENTLEMEN,—

I see that Mr. Lynn expressed his disbelief in the alleged visibility of any of the Satellites of Jupiter by the naked eye at a meeting of the British Astronomical Association reported in your Journal; several members, I see, gave instances in which they were satisfied that they had been so seen, and I would wish to add my testimony to theirs.

I, too, was a disbeliever in this possibility in my early days; but it so happened that when a member of the Trigonometrical Survey of India I had for a short time as my assistant the late Lieut. Elliot Brownlow, of the Bengal Engineers, who, I am convinced, did see some of them without any aid.

One evening, when we were waiting till the signal lamps were ready for observation, Brownlow was watching Jupiter (then in the west), when he remarked that there were some small stars close to it, and, as he was certain about it, I got a telescope on the planet and compared their positions with a rough sketch which Brownlow made: the positions corresponded; and I was induced to repeat the experiment on more than one occasion afterwards, always with success as regards some of the satellites.

This is an old story *, and if I made any notes I have long lost them; but I think I must have mentioned the matter as of interest, though it attracted no notice at the time, and my memory will not carry me back so far as to details.

No doubt the perfect eyesight needed for seeing these small objects in the neighbourhood of Jupiter is rare, but I have myself so nearly approached to seeing bright objects free from the usual rays (which seem to be accepted as the accompaniment of bright stars) by using suitable spectacles, that I think it probable that almost perfect vision does exist: and in Brownlow's case I had quite independent evidence that his vision was very acute; he was a great sportsman, and it was quite well known that very few of the professional shikarries could recognize objects better than he, and we daily had proof that his vision far excelled that of any of the rest of our party.

I am, Gentlemen,
Yours faithfully,

11 Clifton Gardens, Maida Hill, W.
1901, August 4.

J. F. TENNANT.

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* Brownlow was killed at Lucknow by an accident in 1858.

A remarkable Meteor.

Gentlemen—

While watching for late Perseids on the night of Aug. 18 last at $13^{h}\ 24^{m}$, I saw a shooting-star of the 4th mag. moving very slowly along a path of nearly 9 degrees just S. of ε Persei. Its flight was directed straight from Capella, and it terminated its visible course close to ξ Persei, the path being 65° +40½° to 56½° +35°. The duration was estimated as 2 seconds, and the object impressed me as being very exceptionally slow considering its position and direction.

A letter from Prof. A. S. Herschel at Slough was received a day or two later, and informed me that he had observed the same meteor, his observation being Aug. 18 $13^{h}\ 24\frac{1}{2}^{m}$, mag. 3, path 331½° +78° to 306° +57°, duration 1·7 sec.

A comparison of the two apparent courses shows a radiant-point in the Lynx, and the following real paths were worked out indpendently by Prof. Herschel and myself:—

	Prof. H.	W. F. D.
Radiant-point..........	105° +52°	106° +52°
Height at beginning	64 miles.	62 miles.
Height at ending	52 „	50 „
Length of path	27½ „	28 „
Velocity per second	15 „	15 „

The position of the meteor was from over a point near Leighton Buzzard, Beds, to near Watlington, Oxon, and the earth-point was in the English Channel a few miles off Portland Bill, Dorset.

The results corroborate each other very satisfactorily, and prove that the object travelled with abnormal slowness considering the position of its radiant-point, the theoretical speed being 34½ miles per second, while the meteor traversed only 15! I registered about 5 other meteors from exactly the same radiant during the August period, but they moved with normal velocity, being swift and projecting streaks like the Perseids.

The exceptionally slow rate of motion exhibited by this meteor makes one curious to know how it could have been brought about, and how the object came to be apparently mixed up with a system of swift meteors.

Yours faithfully,
W. F. Denning.

Bishopston, Bristol,
1901, Aug. 23.

PUBLICATIONS.

The Constant of Precession.—Herr Ludwig Struve, in Nos. 3729–30 of the *Astronomische Nachrichten*, gives an interesting discussion of the values of the "Constant of Precession" and the Proper Motion of the Sun. The occasion of it is to reconcile his former results with those of Newcomb, an object he has apparently succeeded in accomplishing. When Newcomb published his 1897 results, he pointed the discrepancies between his results and those of L. Struve, especially those derived from discussions of Declinations, and after trying various hypotheses, finally gave up the idea of bringing the two results into agreement.

In the present discussion Herr L. Struve remarks that his work was founded on "Auwers's Reduction of Bradley," the Poulkowa "Positions moyennes de 3542 étoiles," and the mean of the fundamental star catalogues for 1845 and 1855. In the result he found the following corrections to the Struve-Peters lunisolar precession for 1800:—

$$\text{In R.A. } \delta p = -2''\cdot 85 + 1''\cdot 07 \left(\Delta \frac{d\lambda}{dt} + \mu\right),$$
$$\text{In Dec. } \delta p = -2\ \cdot 81 - 0\ \cdot 17\,\nu\,;$$

where $\Delta \frac{d\lambda}{dt}$ is a correction to Peters's planetary precession, and μ and ν constant corrections due to Proper Motion in R.A. and Declination.

According to Newcomb

$$\frac{d\lambda}{dt} = +14''\cdot 36 \text{ for } 1800.$$

According to Peters

$$\frac{d\lambda}{dt} = +15''\cdot 12 \text{ for } 1800.$$

Hence $$\Delta \frac{d\gamma}{dt} = -\ 0''\cdot 76,$$

making in $$\text{R.A. } ap = -3''\cdot 66 + 1''\cdot 07\,\mu.$$

In other words, by merely introducing this correction the differences between Newcomb and L. Struve are reduced. Thus, according to Newcomb, the result for 1800 is 5036″·59, or

$$\text{R.A.}\,p = 5036\cdot 13$$
$$\text{Dec.}\,p = 5036\cdot 89,$$

while Struve's corrected result for 1800 is

$$\text{R.A.}\,p = 5034\cdot 32$$
$$\text{Dec.}\,p = 5035\cdot 17,$$

giving Struve Newcomb, in R.A. + 1″·81,
„ „ in Dec. + 1 ·72.

The difference in the R.A. results is due to a systematic difference of Proper Motion. Newcomb corrects Bradley R.A.s by −0s·079, and the later catalogues used by Struve require a correction of $+0^s\cdot022$, giving $\mu = +0^s\cdot101$ or $+1''515$, and making

$$\text{R.A. } p = 5035\cdot94,$$

a quantity in fair agreement with Newcomb.

This is very simple and satisfactory, but the explanation of the discrepancy in Declinations is not so easy. Herr Struve, however, reduces it by $0''\cdot33$ by proceeding on similar lines to the above, thus leaving an oustanding amount of $+1''\cdot39$.

By a long discussion and placing the stars in order of magnitudes, Struve finally deduces his former result to

$$\text{R.A. } ap = 5036''\cdot31$$
$$\text{Dec. } dp = 5036\ \cdot15.$$

Hence he finally concludes that Newcomb's values cannot be far from the truth. In spite of this, however, he thinks the Paris Conference of 1896 were somewhat hasty in their recommendations, and that it would have been better to wait until results had been derived from a wider range of material. Everything depends at present on the Bradley Catalogue.

THE ASTROGRAPHIC CHART CONFERENCE AND THE EROS CAMPAIGN.—Two publications have lately been received from the Paris Observatory which deal with the above-mentioned subjects, and can therefore be treated in the same paragraph. The first, the Report of the Conference held at Paris last year, contains the *procès-verbaux* and the reports presented to the Conference by the co-operating observatories which have already been summarized (see 1900, July); and besides these there are several appendices relating to the determination of the magnitudes of the stars on astrographic plates by different methods which are interesting to compare. M. Trépied explains that at Algiers a scale of magnitudes corresponding to diameters founded on examination of the diameters of the stars in the Kapteyn-Pritchard typical areas has been adopted, and then with a little practice the observer, measuring the diameter by means of two parallel wires, can at once write down the magnitude with considerable accuracy. At San Fernando, Admiral Viniegra begins with the assumption that all images are ellipses, and then, having measured the axes of the ellipse by help of the co-ordinates which give its position on the plate, he deduces a diameter Δ for each star. From this the magnitude is computed by the formula $m = \text{H} - 0\cdot6\sqrt{\Delta}$ (M.N. R.A.S. vol. lii.). M. Baillaud's method of determining magnitude on a plate by a wedge-photometer has already been mentioned in these pages (July 1901, p. 287). Prof. Donner explains that at Helsingfors the magnitudes are first estimated mentally by the measurer on a scale based on the diameter of the images of the Kapteyn-Pritchard stars, and then corrections are applied to these to reduce them to a common system, the corrections being arrived at by comparison of the images of the same stars on overlapping plates. It may be mentioned, although it is not given in the book now being written of, that the method in use at Greenwich is something analogous to that of M. Trépied, but the diameter is written down instead of the magnitude, and that a comparison similar to, but probably not so refined as, that of Prof. Donner affords a check on the accuracy. The rest of the Report of the Conference deals with the initiation of the Eros campaign for determining the solar parallax, which is now almost ancient history. The latest information relating thereto is contained in the second of the books from Paris—a voluminous Circular No. 8. To this M. Loewy contributes two memoirs—one, on the accuracy of measures of rectilinear co-ordinates, the other on the effect on the measures of the trailed images of Eros, due to its motion, in which he comes to the comforting conclusion that though there may be a systematic error, seeming to depend on the magnitude of the star, the effect of this is slight and would disappear by multiplying the measures. M. Prosper Henry and M. C. André had been charged to make a study of the influence of atmospheric dispersion, which they have done by examining the difference between the mean wave-length of the light from Eros and that from the surrounding stars. They found the difference to be insensible, and that Eros produced upon the plates an image equal in intensity to that of the surrounding stars of the same visual magnitudes. There is also contained in the circular some geometry by Mr. Hinks relating to the determination of the parallax of Eros, which has some points of resemblance with that published in our number for 1900, August, and a paper by Prof. Comstock on the accuracy of the filar-micrometer observations made at Madison. The remainder of the circular is taken up by the detailed reports, in continuation of those in Circular No. 7, of the work done at all the observatories, of all of which the following is a summary:—

	Photography. Number of		Micrometer measures. Number of	
	Plates.	Nights.	Series.	Nights.
Algiers	281	82		
Arcetri	...	...	35	28
Bamberg	...	...	...	46
Besancon	...	...	68	31
Bordeaux	63	30	13	12
Brussels	...	...	6	3
Cambridge	120	39		
Christiania	...	...	63	48
Cordoba	10	3	20	15
Dunsink	25	16		
Dusseldorf	...	...	12	11
Edinburgh	...	...	62	22
Lowell	...	...	47	19
Greenwich	411	97		
Helsingfors	73	48		
Kasan	...	...	25	15
Königsberg	...	...	131	88
Lyons	...	...	98	34

	Photography.		Micrometer measures.	
	Number of		Number of	
	Plates.	Nights.	Series.	Nights.
Madison	...	...	10	10
Marseilles	...	...	112	74
Minneapolis	106	57		
Lick	123	65	48	27
Nice	...	...	137	76
Northfield	73	56	22	21
Oxford	145	50		
Padua	...	...	180	41
Paris	174	68	139	61
Poulkova	27	22	...	64
San Fernando	85	40		
Strasburg	...	...	66	32
Tachkent	26	21		
Teramo	...	...	...	100
Toulouse	53	35	88	46
Upsala	91	30		
Washburn	...	...	63	49
Washington	...	...	91	61
Yerkes	...	...	181	65

This represents the work done at each observatory approximately, but it is difficult to set down the accurate relative value in a table. At some observatories many exposures are given on a plate, at others a single one. It is inferred that at Lick, for example, each plate is exposed only once, and for this reason the number of series of plates is given although the total number is larger. At Greenwich two photographic telescopes were in use simultaneously and plates with each were taken in duplicate, so that the number is proportionately large.

Meridian observations of the Étoiles de Repère have been made at the observatories of d'Abbadia, Greenwich, Königsberg, Lisbon, Lick, Paris, Rome (Collegio Romano), San Fernando, Toulouse, Washington.

Owing to the position of Eros, there was a very short time during which it could be observed at the Cape, and during this period, unfortunately, very bad weather prevailed.

The observatory at Catania has also taken photographs, but definite figures are not given.

NOTES.

Comet Notes.—It is reported that Encke's Comet was observed by Prof. Wilson, of Carleton College Observatory, Minnesota, on Aug. 6, $9^h\ 25^m$ G.M.T., in R.A. $6^h\ 2^m\ 1^s$, Declination $31^\circ\ 43'\ 30''$ N.

A Secondary Sun-spot Period.—Sir Norman Lockyer recently communicated to the Royal Society a paper written by his son, Dr. J. W. S. Lockyer, with the title "The Solar Activity 1833-1900." This memoir, very neatly and accurately prepared, collects the information accumulated about sun-spots during these years and discusses the dissimilarity of the various periods. Though quite appreciating the value of the paper, we scarcely follow the author in thinking that his main point is well established. Dr. Lockyer tabulates the dates of minimum and maximum of sunspots as given by Dr. Wolf and his successor Dr. Wolfer from the minimum of 1833·9 to the maximum of 1894·0. Then having taken differences he finds that the maximum has followed the minimum by a varying space of time, for in 1837 the maximum followed minimum by 3·3 years, in 1848 by 4·6 years, in 1860 by 4·1 years, and so on. Dr. Lockyer then says that these periods between minimum and maximum have their greatest value at every third sun-spot period, or every 34·4 years. In this, as we have said, we do not follow the author with confidence; for, first, he has only two maxima of his secondary curve to judge from; and, secondly, there is always considerable uncertainty as to the exact date of a sunspot minimum or maximum. No doubt Dr. Wolf and his successor assigned dates to their epochs with much judgment, but these epochs are such protracted events that it is difficult to name any particular month as the time of sun-spot minimum or maximum, and of this we have an example in the minimum just passed, if, indeed, it has passed. Other periodic phenomena are discussed in, a similar way, climatic variations, and auroræ and magnetic storms, whose variations are all generally in accordance with a thirty-five year period. Dr. Lockyer quotes Prof. Newcomb's remark in the *Astrophysical Journal* for January last—"Underlying the periodic variations of spot-activity there is a uniform cycle . . . "—evidently with the meaning that Newcomb was referring to some such period as this of 33 years, but it may be questioned whether this view is correct. As to the amount of "spottedness" during a period, the author finds that the total spotted area included between two consecutive minima varies regularly, with a period of variations of about thirty-five years, which differs somewhat from the result of a well-known investigation by De la Rue, Stewart and Loewy, in which they found that the amount of "spottedness" during a whole period was practically constant—that is, when there was a long space between successive points of minimum of the sunspot curve, the intervening maximum was not very high. Finally, considering our Sun as a variable star with a mean period of about 11·1 years, he exhibits a table of the variations of η Aquilæ for comparison.

STELLAR VELOCITIES IN THE LINE OF SIGHT.—The plan initiated, we believe, by Harvard Observatory and followed now by Yerkes and Lick, of periodically issuing circulars giving a *résumé* of recent work, has much to commend it. A specially interesting Bulletin (No. 4) from Lick just to hand collects some recent results secured with the Mills spectrograph; some of these researches have already been published elsewhere, but this summary of similar results is very handy.

The following is a further summary.

These stars have variable velocity in the line of sight:—

π Cephei:	1899 Aug. 23, −36 km.;	1900 Oct. 7, − 5 km.;	1900 Dec. 24, −23 km.
31 Cygni:	1899 July 2, −11 km.;	1900 Aug. 12, − 3 km.;	1901 June 5, + 3 km.
ξ Piscium:	1899 Sept. 19, +27 km.;	1900 Sept. 17, +33 km.;	1900 Dec. 26, +35 km.
τ Persei;	1898 Oct. 26, +10 km.;	1898 Nov. 28, + 8 km.;	1900 Oct. 15, − 2 km.; 1900 Oct. 17, − 4 km.
ζ_1 Ceti:	1897 Oct. 27, − 9 km.;	1898 Oct. 17, − 8 km.;	1900 Oct. 19, + 4 km.; 1900 Dec. 4, + 1 km.
ε Hydræ:	1899 Dec. 26, +43 km.;	1900 Dec. 24, +40 km.;	1901 April 23, +32 km.

δ Equulei is one of the most interesting double stars known. Its period of revolution was supposed to be 11·4 years. It was observed with the Mills spectrograph in 1900 June, when it was found to have a radial velocity of −14 km., but no peculiarities, such as doubling of the lines due to the two components, was apparent, which was to be expected, as both components were then moving at right angles to the line of sight. Since then, from observations made at Lick by Mr. Aitken, Mr. Hussey has shown that its period is probably 5·7 years.

A few negatives of the star were obtained last spring with the Mills spectrograph, in which the spectra of the two components are plainly shown with displacements corresponding to a relative velocity of 35 km. If the distance between the corresponding lines decreases and vanishes in the next few months, it will be conclusive proof that the period of the star is near 5·7 years: otherwise the longer period should have the preference.

There are also given in the Bulletin some more facts about Polaris. It will be remembered that in the autumn of 1899.* Prof. Campbell announced that his spectroscopic observations showed that Polaris was a triple star, that the bright star which we see was moving around the centre of mass of itself and an invisible companion, and that this system moved with a variable velocity as though it wete moving as a whole about some other star as centre. Observations have since been made to verify the variable velocity. In 1896 the velocity of the centre of mass of Polaris and the dark component was, it is said, −18 km. per second, in 1899 it was −11·8, and now it is given as −13·5. The period of the binary system $3^{d}\ 23^{h}\ 14^{m}{\cdot}3$, deduced in 1899, seems to satisfy recent observations perfectly.

Prof. Campbell also gives a series of six observations of θ Ursæ Majoris between 1897 November 16 and 1901 January 27, which do not indicate variable velocity, but a steady velocity of +15 km. per second, and also some observations of δ Orionis by Mr. Wright, which confirm the discovery of variable velocity of this star made by M. Deslandres:—

δ Orionis: 1900 Aug. 12, +3 km.; Aug. 21, +51 km.; Sept. 17, −69 km.

The determinations made at Lick of the radial velocity of

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* *Observatory*, vol. xxii. p. 379.

α Persei and of Groombridge 1830 have already been noticed in recent numbers of this magazine (May and July).

LARGE VELOCITIES IN THE LINE OF SIGHT.—The following instances of stars which have large radial velocities were given by Prof. W. W. Campbell in the *Astrophysical Journal* for 1901 January. These figures are the means of determinations by more than one observer, so that they may be relied on:—

ϵ Andromedæ	−84	km. per sec.
μ Cassiopeiæ	−97	„
δ Leporis	+95	„
θ Canis Majoris	+96	„
ι Pegasi	−76	„
μ Sagittarii	−76	„

The + sign denotes recession, the — sign approach.

STATIONARY METEOR-RADIANTS*.—The radiant of a shower of shooting-stars is the point or area from which all the stars appear to move when perspectively projected on the celestial vault. If the tracks of a shower of meteors are laid down on a star map, and if these tracks are prolonged, all of them will intersect in a point, or, at least, within a small area—the radiant. The meteors are really moving in parallel straight lines in space. Their paths are perspectively projected into great circles of the celestial sphere, and have a common vanishing-point. The case is easily understood by that of the 'Sun drawing water,' which is often seen about sunset. The rays of the Sun are really parallel, but they seem to radiate in all directions from the Sun's disc in great circles that have a common vanishing-point.

This perspective theory demands that the radiant-point of a shower of meteors should rise, culminate, and set by the Earth's diurnal motion, precisely as the Sun, or a star, rises, culminates, and sets. The meteors on any night do, in fact, radiate from spots which remain fixed among the stars, and which rise, culminate, and set as do the stars themselves. If the shower continues for many nights (like the Perseid shower, for instance), the place of the radiant usually shifts among the stars, as it ought to do, since its position is due to a geometric configuration which changes as the Earth moves. The perspective appearances change as the place of the spectator is altered by the Earth's motion in its orbit. Mr. W. F. Denning, has stated that he has found cases where the appearances differ from these normal conditions. For certain showers of meteors, the radiant does not change its place among the stars as the Earth moves in its orbit, but, on the contrary, the radiant remains stationary for weeks. A typical case of the sort is the shower of the Orionids. This shower persists for about two

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[*] This very terse statement of the problem of Stationary Radiants is taken from the *Popular Science Monthly*, some slight alteration being made.

weeks (October 10–24), and the radiant remains stationary near the star ν Orionis, instead of shifting with the Earth's motion as the laws of celestial perspective demand.

No certain explanation of such stationary radiants has been forthcoming; and the correctness of Mr. Denning's observations have been doubted on that account. The explanation suggested by Prof. Turner that the paths of the meteors are deflected by the Earth's attraction is given in *Mon. Not. R. A. S.* and is probably well known to our readers. Prof. von Niessl, of Brünn, has also investigated the subject from a geometrical point of view. Given the phenomena and the fact that they are produced by the perspective projection of the actual paths of meteors in space, he has inquired what the paths must be to satisfy all the conditions. If we assume swarms of meteors, moving with small velocities in space, in hyperbolic orbits nearly parallel, the orbits being asymptotic to the Sun, meteors proceeding from such swarms would seem to have a stationary radiant. Moreover, such meteors must originate in certain fixed emissive centres in the stellar regions (beyond the solar system). The phenomena for certain aerolites whose fall has been observed are accounted for by reasonable assumptions as to the existence of the cosmical centres of emission, primitive velocity, and direction. A geometrical explanation of stationary radiants can be had in this way. It is not yet possible to say whether there is sufficient physical evidence to make the existence of such extra-solar emissive centres probable.

NOVA AURIGÆ.—A remark made in our March Number, to the effect that this object had not been seen for many years, would have been more in accordance with facts if we had said that no observations had been published during the last year or two, but it seems to have had the effect of inducing observation of Nova Auriga—or it may be that the appearance of the Nova in Perseus may be the cause. At any rate, Prof. Campbell and Mr. Aitken looked for the star on the evening of 1901 April 4, and found it without difficulty, though the sky was hazy and illumined by the full Moon. Nova Aurigæ was estimated to be of the 12th magnitude on Burnham's scale. Prof. Campbell says in Ast. Soc. Pac. vol. xiii. no. 79 that the same peculiarities of focus existed as in previous years, and no doubt the spectrum is still nebular. His last observation was in 1898, August, when the star was of the 11th magnitude.

PROF. SCHIAPARELLI.—This well-known astronomer has retired after 40 years' service from the Directorship of the Royal Observatory at Brera, Milan. As recognition of his splendid work to science and for his country he has been presented, the effects of a public subscription raised by his fellow citizens, with a gold medal and an album. The medal, 51 millimetres in diameter, bears on one side a picture in relief of the Observatory of Brera surrounded by the motto *Turris siderum speculatoria*, on the other the inscription *All' astronomo Giovanni Schiaparelli, Saviglianese. Concittadini ed ammiratori Auspice il " Corriere di Savigliano"* 1900.

THE Visitors to Greenwich this holiday-season have not been so many as in some years. The record-book contains the names of Mrs. E. C. Pickering, Miss Gould, Miss Whitney and Miss Furness of Vassar College, Mr. Perrine of Lick, and Mr. Milham of Williams College, Mass.

NOVA PERSEI.—The new star in Perseus is now well situated for observation. About the middle of August it appeared a little brighter than magnitude 6, and on the nights when it was observed there was no trace of redness. On Aug. 12 and 15 it was of a decided steel-blue colour. We have been informed that its spectrum observed at Lick on July 12 was of the planetary nebula type.

ERRATA.—August number, p. 309, line 34, *for* 1826 *read* 1846; p. 310, line 45, *for* Thompson *read* Thomson; p. 325, line 10, *for* Moureau *read* Moureaux. The note on p. 324 about Col. Knight's thermometer is likely to cause misconception. The thermometers have not *very long* stems, but are sunk in tubes, the bulb of one being at the depth mentioned. In this note, *for* "longest" *read* "deepest" in two places.

WE are glad to announce that Mr. Maunder has returned safely from Mauritius, and has quite recovered from his severe attack of fever.

We believe Mr. Newall also returned last week on the 'Oceanic.'

FROM AN OXFORD NOTE-BOOK.

A PAPER by Prof. R. S. Woodward in No. 502 of the *Astronomical Journal* gives modern estimates of the "Effects of Secular Cooling and Meteoric Dust on the length of the Terrestrial Day." The author remarks on the neglect of the first of these problems since Laplace's time; and solving it afresh with modern data, finds Laplace's conclusion as to the smallness of the effect confirmed and capable of extended application. "It appears safe to conclude that the length of the day will not change, or has not changed, as the case may be, by so much as a half second in the first ten million years after the initial epoch." As regards meteoric dust, at the rate of accumulation assumed it would take a million million years to produce a change in the length of the day as great as a quarter of a second. These figures are on a scale which geologists will thoroughly approve.

VARIOUS observatories and individuals have lately received a regular succession of Meteorological Reports from the Abbassia Observatory near Cairo. They are indications that this Observatory has taken a new lease of life under the vigorous directorate of Capt. Lyons, R.E., who is engaged in moving the Observatory to a new site some 15 miles south of Cairo. A general reorganization is proceeding at the same time which, though at present it has not extended much further than the meteorological and seismological observations, will soon reach the magnetic work and (it is to be hoped very shortly) the astronomical. A great deal could be done in the fine climate of Egypt by an active astronomer. The next accessible * Total Eclipse (1905 August 30) runs across Egypt; and though the line may be reached by the shorter voyage to the Pyrenees, the certainty of fine weather in Egypt will doubtless attract observers thither. In such case it will be a great thing to know that able assistance from Cairo can be reckoned on. Four years is plenty of time to look ahead, but for some things not too much.

THE Reports of the Cape Observatory take a prominent place among such documents. In the Report for 1900, Sir David Gill announces a new transit-circle, with observatory and new form of clock; the wiping off of all arrears of reduction and publication due to his predecessors; gifts of several new prisms from Mr. McClean; and a project for the survey of South Africa from the Cape to Cairo (which is, of course, partly done already), besides other matters too numerous to mention in a single paragraph. Of these last, however, the determination of "magnitude equation" in transit observing may be noticed. Gauze screens over the object-glass were used to reduce the light of a star by 1·8 or 3·6 magnitudes; and the magnitude equation for 6 observers determined independently. The observers differ sensibly, and the mean results are about half those indicated for Greenwich (by inference from Cambridge, see *Mon. Not.* lx. p. 11) by the photographic method of determination. In the reference just cited reasons are given for suspecting that the "screen-method is liable to under-estimate the amount of the equation"; and in view of the importance of this matter it is much to be desired that the Cape observers should be tested by some method independent of screens.

GOVERNMENT regulations about retirement from office work in curious ways. At the present moment our greatest living astronomer has been superannuated, which looks at first sight like a great disaster: but there are compensations. Retirement from office undoubtedly sets free a certain amount of time, and with a man like Newcomb that time is bound to be used for the good of astronomy in some way. I will even hazard the opinion that possibly Newcomb's leisurely criticism may be of more value to astronomy generally than his concentrated official work. This is a high estimate, but I make it deliberately. We get far too little criticism of a high order on work done; most of the best men are too busy with their own work to examine that of other people carefully. And yet criticism is badly wanted, as I hope to show by the instance which gives rise to these reflections.

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* *i. e.* omitting that of 1904 in the South Seas, which few will have time to visit.

In No. 500 of the *Astronomical Journal*, Newcomb gives "A Study of the Limiting Magnitudes of the Cape Durchmusterung." This great work, dealing with nearly half a million stars, is discussed in two of the three great volumes it fills by its distinguished authors, Sir David Gill (in an Introduction of 122 pages to Vol. I.) and Dr. J. C. Kapteyn (in an Introduction of 67 pages to Vol. III.); and one of the conclusions reached in these discussions is that, even under a cloudless sky, the variations in the steadiness and photographic transparency of the atmosphere are so great that the faintest stars photographed may vary in brightness by so much as a magnitude and a half. Very well! Now Newcomb picked up these volumes the other day and found the work "remarkable for the clearness and fullness with which the data on which it is founded are presented," which features render it a "very attractive subject of study, and at the same time facilitate such corrections as farther research and comparison may show to be necessary." And the main conclusion he arrives at in the paper quoted is in almost direct contrast to that just mentioned about limiting magnitudes. He states in italics his conviction that the authors of the *Cape Durchmusterung* would have been nearer the truth if they had assumed that the limiting magnitude with a given exposure is the same on all plates, an assumption which they deliberately rejected. Now it is not my intention to intervene in such a discussion. "Where doctors differ, let angels fear to tread," or words to that effect. We shall hear more of the subject no doubt; and from the discussion which ensues we are bound to learn something on a matter which concerns us all. But what I wish to draw attention to is this. Here is a vital point in astronomical photography; and people of acknowledged repute differ fundamentally about it while interpreting *the same* observations. Does this not suggest that we spend far too little time in discussing existing observations, and are in too great a hurry to make new ones? There seems to be a chance that we may now get some light on this important point; but should we have heard anything of the other side of the question if Newcomb had not been available for criticism? The natural course is to take the conclusions of Gill and Kapteyn almost without examination; and the event may yet prove that they are right. But as things stand at present there is certainly a case against them of which we have only heard because a great man has leisure. Let us then pray for more leisure for those who can use it in criticism. "If things go on at the present rate," said a President of a British Association Section, "a man will soon have barely time to read the papers he writes himself." Astronomers used to make the mistake of accumulating observations which they never reduced: things have improved a little—the observations are reduced and published, but are they properly read and digested?

WITH reference to the last note of the August number, I have now heard again from the Editor of the *Astrophysical Journal*; and I wish to correct a mistaken impression which my former words may have caused. In May last (p. 215) I wrote:—"He [the Editor of the *AP. J.*] has struck out a line with which I have the heartiest sympathy—that of collecting into one channel most or all of the papers connected with Astrophysics."

These words doubtless convey the impression which I must frankly confess I had in my own mind—that *many* of the papers (if not most of them) were collected, *i.e.* reprinted from other sources. It was an impression based, as impressions often are, on one or two instances which had caught the eye; and as I find on proper examination that it is a mistaken one, it seems proper to correct it fully. Fully 80 per cent. of the articles published are original, as any one who takes the trouble to count may verify. More exact figures can of course be given by the Editor himself, and in fact he did send me a very complete and definite statement; but it was such a statement as he might well prefer not to have published; and on referring to him I found that this was indeed the case. He would be glad to have it pointed out that "the journal devotes considerably more space to original articles than to reprints" (which I hope is now clear), but deprecates anything further; and though I feel sorry that his most interesting letter will not reach others, this of course ends the matter.

THE Smithsonian Institution is preparing a "List of Observatories," and has sent out proofs for correction. The copy which has reached me suggests two questions. First:—Is such a mere list of observatories, without details of staff or even directors, of much use? And secondly:—Is not the list rather carelessly prepared, even for a proof? But it is ungracious to find fault with it, for it has revealed to me the existence of another Oxford, with a University Observatory all complete, in Mississippi. May we exchange greetings?

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIV. OCTOBER, 1901. No. 310.

The Meeting of the British Association at Glasgow.— Address to the Department of Astronomy by Professor H. H. Turner, *President.*

It was hoped, as you are doubtless all aware, that this Chair would be taken by the Astronomer Royal for Scotland, Dr. Copeland; but unfortunately illness has prevented him coming to the Meeting. In doing what I can to fill his place at very short notice, I shall not attempt, nor would you expect, a formal address such as we hoped to hear from him; but I will venture to put before you one or two reflections on a topic which has been much before my attention during the last few years because directly connected with my own work, and which has a special interest for us from the allusions made to it yesterday morning by the President of our Section, viz., the question of scientific co-operation. It is a matter of considerable importance to astronomers who have to deal with numerous observations and calculations; indeed, the millions and billions which express the distances, sizes, or ages of the heavenly bodies, and which are used to such good purpose by some lecturers for startling the imaginations of their audiences, scarcely surpass the numbers which must be used to express the work to be done by an astronomer. The enterprise on which we are engaged at the Oxford University Observatory at the present moment is the measurement of a quarter of a million star-places, which will take us about seven years; and we are only one of eighteen observatories co-operating in a scheme of work. The product of eighteen by a quarter of a million does not bring us near the billions; but if we are minded to produce big numbers we might remember that in the determination of each individual star-place a good many figures are required. At Oxford we try to keep the number to the irreducible minimum, but it certainly exceeds thirty even there, while at other observatories it reaches 300 or 400. Thus we can with ease secure a creditable position in the thousands of millions in respect of this one piece of work, and the lapse of a century or two is all that is necessary to produce billions of figures in the ordinary course of astronomical observation. It is clear that in such work co-operation is an all-important factor, and the study of the best means for securing it and for using it when secured may well claim a share of our attention.

I may pause for a moment to consider the possibility that our experience may be of value to the devotees of other sciences. "Other sciences," said Major MacMahon yesterday, "are not so favourably circumstanced as is Astronomy for work of a similar kind undertaken in a similar spirit." But what may be true to-day may not be true to-morrow. It was not astronomers, but mathematicians, who first showed the value of a certain kind of co-operation. Major MacMahon reminded us that the Spitalfields weavers founded a mathematical society in 1717, and thus anticipated by more than a century the formation of the Astronomical Society in 1821, which ultimately absorbed its prototype. Possibly in the future mathematicians will find the need of co-operation of this other kind, which consists in sharing a great piece of work among several workers for the sake of comfort and rapidity, and so may profit by our example, as we formerly profited by that of the Spitalfields weavers. And there are indications that in another science, that of zoology, the time may be close at hand when co-operation between workers of a type very similar to that in full swing in Astronomy will be a boon, if not a necessity. Professor Karl Pearson, Professor Weldon, and others are introducing into zoology numerical operations on a large scale, which promise further and further increase; and they would no doubt be ready to indicate even now enterprises of a valuable kind which they are only deterred from undertaking by their magnitude, and which a suitable scheme of co-operation might bring within the range of practical politics. Hence we should look to our methods of work in Astronomy with the responsibility attaching to those who are leading where others may follow; and above all things take care to make clear any mistakes we have made, so that others may perhaps profit by our experience.

If it seems invidious thus to emphasize our mistakes, I would remind you that astronomical co-operation has not always been successful; indeed, it has very often ended in failure. I do not mean simply failure to attain its object. The band of astronomers who divided the sky between them at the end of the eighteenth century to look for a minor planet met with this kind of failure, for the first discovery fell by the irony of Fate to another who was not engaged in any special search of the kind. This unlucky accident must not, however, make us forget that the co-operators worked diligently side by side for several years. Failure of a more real kind has overtaken enterprises to chart the stars or to map the Moon, which have proceeded very little further than the preliminary organization. Some workers have dropped out early in the history of the scheme, some have not even started, and the blanks have not been filled up; sooner or later—generally sooner —the scheme has been abandoned. The curious may read of some of these schemes in back numbers of the *Monthly Notices*, though some of them never get into print, and are only to be traced in the Minutes of the Royal Astronomical Society. And yet of many of them, if not all, it may safely be said that a little more energy on the part of *somebody* would have produced an assured success; somebody to see that the gaps were filled up, and dilatory workers hastened or superseded; somebody to be a sort of foreman of the works. It does not seem unlikely that this general supervision is best performed by one not actually engaged in the work himself —a man of affairs. One of our great London schoolmasters declares that a nominally idle man should be at the head of all enterprises; that he never knew any good come of any work where there was not "a man with his hands in his pockets looking after it." We have scarcely found this to be a necessity in Astronomy; for the men who have looked after the eighteen observatories taking part in the Astrographic Chart have been Directors of the Paris Observatory—men with many things to claim their attention, and to the individual energy of the late Admiral Mouchez and his successors the work owes a great deal.

But the removal of this danger draws our attention immediately to another—that of taking far too long in finishing the work. The project for making the chart was originally discussed fourteen years ago, in 1887; and it was urged by many of those present that a reasonable time, say ten years, should be fixed for the completion of the whole. In spite of the representations of this prudent minority, the programme was made an ambitious instead of a modest one, and some stretching has been done since, with the result that after fourteen years only one or two observatories are within sight of the goal, the majority seeing from ten to twenty years' work ahead of them; and there were three which did not begin, whose places have had to be filled. With this experience we may well ask whether the limit proposed even by the prudent minority was not too high, and whether it would not be well to fix five years as a limit to any scheme of co-operation which is as yet on paper only.

The danger of attempting too much is illustrated in a somewhat different way by the Eros campaign. It will be clear from what has been already said that the eighteen observatories responsible for the Chart have their hands quite full; and now comes a special occasion—an opportunity that will not occur again for thirty years —to determine the solar parallax. Last winter the newly-discovered planet Eros was known to be coming close to us, and we had an occasion of more value than the Transits of Venus. What were the eighteen observatories to do? They could not at any rate refuse to take photographs, and this has been done; even this meant a great deal of additional work for some people for a few months; but it is a mere trifle compared with the work that is still to come in measuring and reducing the plates, which will be a sensible fraction of the work already projected for the Chart. Which is to be done first? Prudence suggests finishing one enterprise before beginning another, putting aside the Eros plates until the chart work is finished. On the other hand, there are thirty other observatories sharing the Eros work with the original eighteen, and they will be more or less impatient for our results. In this dilemma some rather unsatisfactory compromise will no doubt be adopted; but we may heave another sigh that the advice of the prudent minority in 1887 was not taken, for in that case not one or two but many of the eighteen observatories might have completed the Chart work before Eros came.

I now pass to a different kind of danger to which co-operation renders us liable. To ensure homogeneity in the work it is necessary to bind the associating individuals by certain rules, and we run some risk of checking that originality which is almost vital in scientific work. There is scarcely any scientific operation so mechanical that it may be safely left in entire charge of those without originality and the liberty to use it. Quite recently a scheme of co-operation has been adopted in the preparation of the nautical almanacs of the different nations. It is thought that certain calculations to be performed are so well settled that independent calculation is a needless waste of labour, and thus certain sections of, say, the American Nautical Almanac and our own will be henceforth identically the same, printed from the same manuscript computations. I cannot but regard the project with some alarm. The risks against which we are guarded by independent computation may be small, but I cannot believe them to be evanescent, and I attach some value to the healthy stimulus of comparison (or we may perhaps say competition) even for nautical almanacs. Differences revealed by such comparisons in the past have often been traced to causes which were by no means obvious or unworthy of attention.

But without laying too much stress on this case, which is obviously an extreme one, we can, I think, well understand how the taking part in a co-operative scheme may lower the tone of scientific work. There is a very real possibility of replacing the alert spirit of investigation by a mere mechanical regularity; nay, even of making one who should be an astronomer into a mere drudge. We can only too easily imagine a scheme of work in which the rules are laid down so completely and so stringently by the central body that nothing is left to the initiative or originality of the individual observatories; and the Director of such a one might find himself with nothing to do but see that the rules were adhered to. If the work were at the same time planned to extend over a period of ten or twenty years, as is quite possible in Astronomy, we can well understand that his efficiency as an intelligent scientific worker might become seriously affected. We must not shut our eyes to this danger. Astronomical work is terribly liable to settle down into routine, as we all know, and the existence of so many small observatories where nothing is done beyond routine observations with the transit-circle is not a credit to us. It is reassuring to find that many of them are ready to use opportunities which present themselves. For instance, when the Eros work was planned, fifty observatories responded to the call for volunteers. But is there not even here another point of view? What were all these observatories doing before that they were able so readily to take up a new project? Some of them we know had enough on hand already, and only added the Eros work with reluctance; but it is to be feared that others hailed it as a welcome opportunity to do something of some use, not having been able to think of anything for themselves. This thinking of what one's work is to be is, of course, the hardest part of research—devising something to do that shall be a real step in advance. Some fortunate men find it comparatively simple, but to the majority it is a labour and toil, and only through much tribulation do they enter their kingdom—their own domain in which they recognize their own true work. It is much easier for such to turn aside and follow some king who has come to his crown more easily; to take a share in a great piece of work organized by some master-mind. But is not this a serious loss to them and to science? May not schemes of co-operation kill the originality of the humbler thinkers by removing the incentive to independent thought?

Here, however, I end, for the present, at any rate, my list of the risks and dangers which co-operation brings in its train. It is time to turn to the other and brighter side of the matter; for there is a brighter side which presents itself, as it should to experimental philosophers, when we come to practical working as opposed to forecasting; and it is because the great scheme of the Astrographic Chart illustrates vividly both the dark side and the bright, both the possible evils of such schemes and the actual benefits which may replace them under certain circumstances, that I have ventured to select it so often for reference in these remarks. We have seen how it has escaped the premature decease which has befallen other such schemes, owing in great measure to the energy of the central authority. The mistake of attempting too much is unfortunately now irremediable in this particular case; but it may serve as a warning on future occasions. It remains to show how the danger of crippling individuality has been averted in an unexpected, almost a comical, manner.

At the outset this danger was distinctly threatening. At the earlier conferences there was manifest anxiety, chiefly on the part of those who were not going to do the work, to bind down the workers rather stringently by rules of procedure. The anxiety seemed to be intensified rather than diminished by the circumstance that it was not very clear what these rules ought to be.

Where several courses were open, each found its champion, and the discussion was perhaps most animated in the cases where the teaching of actual experience was least available. On several occasions a decision was only arrived at by an expedient which seems to be familiar in Continental meetings, but is little known in England; perhaps it deserves a wider recognition. When formal discussion waxes warm, the President declares the meeting dissolved, for ten minutes of informal conversation. The meeting forthwith breaks up into animated knots of eager talkers; opponents who have been addressing one another with the meeting between them rush across the room to each other and put their points with renewed emphasis and unfettered gesture, and for ten minutes there is apparent confusion and some noise. But when the President's bell again rings, the effect of the outburst is manifested in a restoration of sobriety and the passing of a resolution; and so the number of resolutions mounts up, and by the end of the Conference a respectable list of them is ready for the printer; a list quite long enough to quench any spark of originality in the individuals taking part in the work. But now comes the unforeseen feature of the enterprise. The participating workers go off to their observatories with a copy of these rules in their pockets, and do not observe them. Such as they find convenient they adhere to closely; but when they find by experience that a rule will not work, they do not hesitate to prefer their experience, as good and faithful experimental philosophers should. And their individual experiences were by no means similar, so that the sheet of rules was torn across in all sorts of directions; the original copy would be now barely recognizable by those who subscribed to it.

But then is anything left? Is not this the practical failure of the scheme? On the contrary, it was its salvation. The diversity of experience was not fundamental, but to a great extent apparent only. The rules which were broken were those which experience proved non-essential, and which ought never to have been made; and when those who had actually carried out a considerable portion of the work met in 1900, they found that they had arrived at practically the same conclusions by a diversity of routes. It was inevitable that they should, rules or no rules, if they went to work honestly and perseveringly; and if some went a longer way and some a shorter to the same goal, this was, after all, an unimportant matter beside the fact that they all arrived at last. Had they not thrown off the needless constraints they might never have arrived at all.

But the comedy of this result has a very serious significance. We may heartily congratulate ourselves that the time is not yet come when astronomers are prepared to lose their individuality in a co-operative scheme of work; and still more that such schemes can be found where such loss of individuality is unnecessary. May it not be said that something very similar has been realized in the case of the other scheme of co-operation referred to by the President of the Association yesterday, the scheme for a Catalogue of Scientific Literature? The original proposals were of a kind which left too little scope for the individuality of the different sciences. Fortunately the mistake was rectified promptly, and the present plan leaves much more to individual judgment. Some such compromise would seem to be essential, if we are not generalizing too hastily, to the success of co-operative enterprises in science. We must, above all things, take care not to crush individuality. I would even go so far as to say that so much of the element of *competition* as can be preserved without endangering uniformity in essentials should be diligently cultivated. Add that the original scheme should be as modest as possible, and that an energetic man should be put in a position to wake up the dilatory and to ensure that the pace, which is necessarily that of the slowest, be not funereal, and I venture to think that we may eliminate failure from co-operative scientific enterprises.

Prof. G. Forbes made a communication relative to the "Position of a Planet beyond Neptune." The author had previously called attention to the fact that there is a group of cometary aphelia at a distance from the Sun about 100 times that of the Earth and that there are similar groups at the distances of Jupiter and Neptune, and that analogy suggests the existence of an exterior planet. The present paper stated that a search made for this had been unsuccessful and also pointed out that given the existence of such a planet the comet 1844 III. or 1843 II. might be a return of the lost comet of 1556. Mr. W. E. Plummer joined in this discussion.

Mr. A. R. Hinks contributed two papers. In the first of these, with the title "The essentials of a Machine for Accurate Measurements of Celestial Photographs," the author attempted to define for a machine to make measures of the highest accuracy, (1) the absolute essentials; (2) the semi-essentials, which soon pay for themselves in ease and rapidity of working. The type of machine discussed was that in which a *réseau* square is projected on to an eyepiece-scale and thereby subdivided to hundredths, while further subdivision to ten-thousandths is effected by a micrometer-screw. The point of application of this screw is the chief factor in determining the cost of the instrument. The author suggested that is should be applied to the microscope objective. He had had the advantage of talking over a machine on this plan with Mr. Horace Darwin, who, in the discussion following the papers, sketched a design which he thought would be efficient and very simple to make.

In his second paper, on "The Possibility of Systematic Error in Photographs of a Moving Object," the author discussed a number of plates of Eros taken by him at Cambridge, (*a*) with stars fixed and planet trailing, (*b*) with planet fixed and stars trailing. He concluded (1) that if the possible systematic error is independent of the star's magnitude, the two series would be rigorously comparable with one another, but each would give the absolute position of the planet affected with the same error; (2) that if, however, the error exists it is sure to vary with the magnitude; (3) that a comparison of the two series of plates gives no such differential error; and (4) that the absolute error is therefore probably insignificant also.

The Rev. A. L. Cortie made a communication on the question whether faculæ share with sun-spots the anomalous law of rotation discovered by Carrington. The observations made at Stonyhurst show that the faculæ follow the same law of drift as the spots.

At another meeting during the proceedings of the Association *Lord Kelvin* delivered an address on "The Amount of Gravitational Matter in the Universe"*.

The Comet and the Eclipse.

THE question "Can one have too much of a good thing?" is usually answered in the negative; and certainly our first feeling at the Royal Alfred Observatory, when we received a note from Captain Nicholson, of the '*Clan McAlister*,' to say that he had observed a bright comet near the Sun early on the morning of April 24, was to think that what with that and the coming eclipse we were well set up in astronomical rarities for a good while to come. Captain Nicholson, having obtained a second observation, it was clear that the comet had been approaching the Sun, and that it migh t looked for next, in all probability, on the opposite side to that where it was first seen. Evening after evening we kept our "weather eye lifting" in case the required newcomer should put in an appearance; not that we were able to give much time to any search for him, as our hands were more than full with our eclipse preparations. Suddenly, on the evening of May 3, the wanderer revealed himself—a beautiful, bright, silver streak on the glowing evening sky; a comet of the very first order.

There is always something to keep man here below from perfect happiness—some crumpled roseleaf under the bed. There were two or three in our case. We should like to have given our visitor the undivided attention which his noble appearance merited; but unfortunately we had a pressing engagement a fortnight ahead that had to be provided for, and we had no spare time on our hands. Then our instruments, which were already set up in readiness for the eclipse, commanded only the eastern horizon, and were snugly nestled down under the shelter of a tall grove of trees to the west, which effectively occulted the comet. It was therefore impossible to reach it either with the Mauritius photoheliograph or the Greenwich coronograph, or (most tantalizing loss of all) the prismatic camera, the three principal instruments of the official outfit. Fortunately the 6-inch equatorial of the Royal Alfred Observatory could reach it over the tree-tops, and so could the Newbegin telescope, which by this time had been mounted in the photoheliograph dome. We at once divided our forces, and tried to bring both instruments to bear upon the object. Unfortunately clouds were already coming up, and we soon lost it for that evening. We were quite ready for it the next night, and got our first photograph of it with the Newbegin telescope. The exposure was necessarily very short, as the background of the sky was so bright as soon to fog the plate, and an exposure of two minutes was found to be about correct. On May 6 Mr. Claxton got its position with the equatorial, and we brought another photographic instrument into action upon it, namely, Mrs. Maunder's little stigmatic lens, which was so placed that it could just get the comet for about half an hour after it became visible in the twilight sky.

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* [We hope to give some account of this in a subsequent number.—EDS.]

Mrs. Maunder has a pathetic remembrance of that half-hour of work. The telescope had only just been shifted to its new position and there had been no time to get it into proper adjustment, the clock was not yet in good driving order, nor had the cameras been brought into equipoise. There was therefore every opportunity for the exercise of a good deal of skill in the delicate work of "hand driving." Add to this there were mosquitoes. We have been recently assured by high medical authorities that all well-conducted mosquitoes observe a strict close time for humans from sunrise to sunset. No doubt they do so in the Roman Campagna, and in other old established centres of civilization, but they don't at Pamplemousses. Nevertheless they do observe this convention to this extent, that they throng forth in their thirstiest millions after the Sun has gone down; and the observer who had one hand on the Right Ascension rod, the other on the Declination, and one eye at the guide telescope—fulfilling the condition for absolute rigidity that three points should be fixed—was absolutely at their mercy. Nevertheless the photograph was taken.

The evening of May 4 was very fine, and we were delighted to see how clearly the dark rift down the middle of the comet's tail was marked. Mrs. Maunder detected on this evening the springing of another tail, a second tail; that is, if we count the two branches separated by the rift of the original tail as one. At the time, however, we spoke of the two branches as the first and second tail, and this new one as the third or subsidiary tail.

No one else of our party was able to distinguish it on the evening of May 4; but on May 5 it was apparent to the dullest sight, and the comet blazed out a noble capital V of light. Night after night this new tail spread out further and further, and a fainter tail, a fourth, was seen branching out some way down the original one.

Our third and last photograph was taken by Mr. Walter with the rapid rectilinear lens of the official expedition, which for this purpose was mounted side by side with the Newbegin telescope on the equatorial stand of the photoheliograph. After this the comet moved somewhat rapidly northward and consequently was in a less and less favourable position for observation. But one thing struck me as very obvious and exceedingly interesting; that was, to see the way in which the plane of the tails of the comet became more and more foreshortened. Night after night it was quite easy to notice the gradual narrowing of the black rift between the first and second tails, and the increasingly acute angle which the third tail made with these.

On May 6 we received a "Science Observer" code telegram from the Centralstalle, Kiel, informing us—so much we could decipher—that at Arequipa a great comet had been discovered some time in May, and adding cipher words, probably to indicate its place. As we had no copy of the code, these latter particulars were unintelligible to us, but we inferred from the telegram having been sent at all that the Arequipa observation was the first known in Europe, and we therefore telegraphed to the Astronomer Royal that Captain Nicholson had discovered it as early as April 24, and Mr. Claxton writing an account of the comet spoke of it by the name of its supposed discoverer as "Comet Nicholson." Hence were tears. The entire Creole press of Mauritius—and Mauritius boasts some fifteen daily papers—rose like one man and denounced him. "Was there not a French steamer, the '*Irraouaddy*,' then about to enter the Indian Ocean?" "Might not its captain, or perhaps its cabin-boy, have seen the comet?" "Were there no Frenchmen in Madagascar or Bourbon?" "Might not one of them have discovered it?" "At least give them the benefit of the the doubt." Months after the comet had passed from sight, ever and anon one of the Creole papers would burst out with a flaming leader against the iniquity of "these Englishmen," who, not content with grabbing everything in Europe, Asia, Africa, America, and the islands of the sea, were grabbing the heavens as well.

So passed our first great astronomical sensation, leaving something like a Tantalus feeling behind it. Such a noble and so rare an object had been presented to our view—by far the finest comet since 1882,—(I am inclined to think the finest since 1858),—and we could do so little with it.

We were more fortunate with respect to the second great astronomical sensation falling to our lot—the eclipse. For the first time since we landed in Mauritius, the eastern sky was quite clear on May 18 at eclipse time, 7.54 A.M. local time. It is true we had our anxieties; for soon after sunrise a heavy cloudbank rose in the east, overtook the Sun, and completely hid it at the time of first contact. It passed, and was followed by patches of light scud, which grew sparser and thinner, until, two or three minutes before second contact, the entire eastern half of the sky was completely clear. I was watching the lessening arc of sunlight on the ground glass of the coronagraph, a beautiful method for showing the phenomenon. The Sun's limb was in much agitation, and the narrow silver arch seemed like a line of boiling mercury. A ghost-like glimmer began to quiver over the glass, throwing up into relief the complete black disc of the Moon; then the bow of sunlight broke up into little molten drops, which seemed to evaporate and vanish, and with the darkness which fell at their disappearance the corona shone out.

It was the fourth corona that I have seen. If my recollection serves me aright, the darkest of those four eclipses was that of 1886. I cannot decide between 1898 and the present one, but the general illumination in 1900 was certainly greater than in any of the other three.

In comparing the actual coronæ, so far as memory may be trusted, the brightest seemed to me to be that of 1898, the next, 1886, and the present one the least bright. The coronæ in the two first eclipses were, according to my recollection, white, but somewhat of that steely blue whiteness which we associate with the electric arc: the two later coronæ, and particularly that of 1901, seemed to me white, but of a softer warmer whiteness, more like the glow of an incandescent lamp; there was a suspicion of a yellowish tinge.

As to general character, the two earlier coronæ are marked off very definitely in my recollection from the two later. The two first were very complex, that of 1886 in particular, full of definite structure; the two last were much more simple and showed much fewer definite lines within the structure, so far at least as that was visible to the naked eye. One or two of the boundaries of the corona of 1901 were exceedingly distinct. But all round the Moon, and beside that amount of coronal light which assumed some definite shape, there seemed to be a faint diffused glow, fading off gradually and evenly into the light of the sky in all directions. Mrs. Maunder, who was examining the corona through a telescope of 2¼ inches aperture, says that she seemed to be looking at the corona "through a silver mist," an expression which admirably describes the appearance as I saw it with the naked eye.

How much of the "silver mist" was real coronal glow and how much was due to moisture in our own atmosphere I cannot say. The sky was cloudless, but was far from being as clear as we had it in India or Algiers.

"Rain, rain and sun, a rainbow in the sky," might well be the meteorological motto for Mauritius. So bright and frequent is its sunshine, so great its moisture that rainbows seem always to arch it. They are seen every morning and every evening, even in blue and cloudless skies. Lunar rainbows are no rarity, and now, during the eclipse, the observers at Quatre Bornes, watching the eclipse under an apparently cloudless sky but in a smart drizzle of rain, saw a rainbow made by the eclipse itself. Quatre Bornes lying some twelve or thirteen miles from us and much more to the leeward than to the windward, it is clear that there must have been much moisture also above Pamplemousses.

I believe this observation of an eclipse rainbow to be unique. I wish it had been my fortune to see it, for the descriptions which were given me show that it must have had some very interesting features. The three observers all agree that it tapered at the two ends, and one of them had been much impressed by bright lines running through it, particularly a bright pink line. Could this have been the C line of hydrogen from the large prominence?

Owing, no doubt, to the want of perfect transparency in the air, the extensions were not photographed to anything like the extent that they were in the Indian eclipse, and the most successful photographs were those timed specially to bring up the lower and middle corona. I think that on the whole the photographs secured give a better general view of the corona, and especially of the lower regions, than the photographs of any other eclipse that I remember. In particular the series of fourteen photographs which Mr. Walter took with the telescope so generously lent to Mrs. Maunder by Mr. G. J. Newbegin are extremely fine. The plate that accompanies this paper is from a drawing by Mr. Walter from a first examination of this series of negatives.

E. WALTER MAUNDER.

Mr. de Sitter's Determination of Jupiter's Mass.

IN the September issue of the *Observatory* Mr. de Sitter has kindly supplied the information for which I asked, and has defended his treatment of the Cape Heliometer Observations of Jupiter's Satellites, adding that my objections "doubtlessly arise from some mistake on [my] part," and "appear to need very little explanation." In the light of Mr. de Sitter's new information, I will state again the points of my objections, and I believe readers will then have little trouble in deciding whether these are trivial or not, and at whose door the mistakes actually lie.

My first point was that it was impossible to treat as correct the absolute values of the mean distances which Marth associates with particular values of Jupiter's mass, and that anyone who did so carried forward errors into his own work. My ground for saying so was that Marth required these numbers to be taken on faith, saying nothing as to whence he got them, whether he adopted them or whether he calculated them, and, in the latter case, what were his formula and initial data. I showed that they were not adopted from the best authenticated sources; and if they were calculated, I considered it very possible that Marth had employed a defective formula, and practically certain that there were no data he could select that were not liable to serious doubt. Mr. de Sitter has arranged his own work so as to depend upon the correctness of Marth's numbers, so that it is necessary for him to reply to this objection, and he does so by producing Marth's formula and data and recalculating his numbers. He further refers to the assurance in his paper, "Marth's data and tables were very carefully examined and compared with Souillart's theory," as making further inquiry into Marth's numbers superfluous.

It is a remarkable comment upon these assurances that the formula from which Mr. de Sitter has recalculated Marth's numbers treats as zero the perturbation of the satellites upon one another. These perturbations are not large, but they are sensible within the limits adopted in the case of the third and fourth satellites, adding 0″·004 and 0″·026 respectively to Marth's radii at Jupiter's mean distance, or 7 and 23 units of the sixth place of decimals to the logarithms given by Marth. Marth was not entitled to ignore these quantities, nor is Mr. de Sitter entitled to assure us that he has examined Marth's numbers and found them to be correct, when as a fact he has derived them from a formula that is obviously defective.

But this is not the chief point of my objection to Marth's numbers. When we consider what values were open for him to adopt for the quantity $J=\rho-\frac{1}{2}\phi$, involving Jupiter's ellipticity ρ, it is clear that none can be fixed upon which carries any reasonable and sufficient degree of certainty. Mr. de Sitter records on p. 78 of his memoir some of the numbers obtained for ρ by different observers; they range from 1/13·7 to 1/18·1, and he himself concludes that you cannot by any grouping of these observations obtain a result which may not be liable to a correction $\pm 1{\cdot}0$ applied to the denominator. Now the value of $\rho-\frac{1}{2}\phi$ adopted by Damoiseau, Souillart, Marth, and Mr. de Sitter is 0·0247187, and this corresponds to an ellipticity $\rho=1/13{\cdot}492$, which is outside the whole range of values recorded above: if any of the different means collected by Mr. de Sitter had been adopted for ρ in place of this, $\rho-\frac{1}{2}\phi$ would have been reduced to something between 0·0153 and 0·0102, while the extreme value 1/18·1 would reduce it to 0·0058. We shall not over-estimate the effect of this uncertainty if we say that it renders doubtful Marth's logarithms of the radii in the fourth and subsequent places for I. and II., and in the fifth and subsequent place for III. and IV. This is the chief point to which I referred in saying that any investigation must carry forward sensible errors if it adopted Marth's numbers as correct.

These errors are introduced into Mr. de Sitter's work when he eliminates the quantities da_i, which correct the mean distances, by means of the equation

$$3\frac{da_i}{a}=-\frac{dM}{M}$$

in place of employing

$$3\frac{da_i}{a_i} = -\frac{dM}{M} + \frac{d(Jb^2/a_i^3)}{1+Jb^2/a_i^2}$$

—which though not exact is perhaps sufficiently correct,—and thereby introduces the assumption that Jb^2 is liable to no sensible correction. Mr. de Sitter justifies this by saying that the quantity J would be better determined otherwise than from his observations; that the quantity $\frac{1}{3}Jb^2/a_i^2$ adds to the maximum apparent elongations of the satellites only

0″·033, 0″·021, 0″·012, and 0″·007

respectively; and that "as a general rule he has only introduced as unknowns the corrections to those inequalities of which the maximum geocentric effect is above 0″·05."

Now if his observations are incapable of determining Jb^2, they are *pro tanto* incapable of determining the mass of Jupiter; further, it is evident that no one is entitled to make such rules as the above without first showing that they do not systematically vitiate the quantities retained; but the reply is in every respect an unfortunate one, for his final value for the correction to M (1047·7) is only −0·474 (p. 66), so that the corrections to the mean distances of the satellites at Jupiter's mean distance would be respectively

+0″·017, +0″·027, +0″·043, +0″·075;

now these, and not the mass of Jupiter, were the quantities which the observers were called upon to perceive, and only one of them exceeds the quantities which Mr. de Sitter's rule obliterates.

We must not be misled by the apparent smallness of the quantities; they are none of them accidental errors which eliminate one another; they are the systematic residuum upon which the discussion turns, and unless they can be dealt with successfully it is useless to try to correct the mass of Jupiter by such means. That quantities even smaller can be derived with much certainty from observations well made and well combined I should be the last to deny. Otherwise we might as well write *Finis* to astronomy at its present stage. But in order to do so it is essential to clear the equations utterly of all the other sources of error than can possibly interfere with them, and it is fatuous to expect them to declare themselves correctly after ignoring equal and greater amounts that mingle systematically with them.

The four mean distances are perhaps the most valuable result to be got from heliometer observations, for all the other quantities can be found otherwise; and it is to be observed that if Mr. de Sitter had taken the obvious course, which was followed by his predecessors, of treating the mean distances employed in his trial ephemeris (*i. e.* Marth's) as liable to independent correction, no trouble would have arisen; neither M nor Jb^2 would have figured in his solution, and if there was difficulty in perceiving a correction to, say, the mean distance of I., that would have declared itself in a larger probable error. Doubtless the corrections would not have obeyed the condition which Mr. de Sitter had tacitly imposed upon his own results — that when calculated with Souillart's constants they should all be consistent with one mass of Jupiter; but the inconsistencies would have been instructive and would very possibly have pointed to a better determination of J than Mr. de Sitter has allowed himself to expect. But Mr. de Sitter will not admit that such a course was open. "If Marth's mean distances had not been consistent with his adopted mass," he writes (*Obs.* p. 344), "evidently the best and the only course for me would have been to make them consistent, and then to proceed exactly as I did in my paper, viz., to eliminate them by expressing their corrections in terms of the correction to the mass. The only assumption which underlies this elimination is, that the mean motions are known with such accuracy that no real corrections to them could be derived from the Cape observations." On this we may remark that Marth's mean distances are inconsistent with his adopted mass and Mr. de Sitter has not corrected them; that his elimination involves the assumption that the correction to Jb^2 is negligible; and, lastly, that a course which is the only one is not entitled to be called the best. But why is it the only course when he had but to leave the corrections to the mean distances uneliminated and to treat them as independent quantities throughthat his arithmetical work? Or why is it even the best when it introduces the quantity Jb^2, which Mr. de Sitter says he cannot determine, and whose neglect involves quantities greater than the correction to M?

In my former paper on the orbits of Jupiter's satellites I attempted to show how many misfortunes had beset those who set out to correct those data; and Mr. de Sitter, the latest voyager, has not escaped the rocks. When the channel is littered with wreckage from so many good ships it is natural to ask with some stringency of each new comer whether he has in fact done what he professes and brought all his cargo safely with him; and, at least in one important matter, I say again that Mr. de Sitter has failed.

Observatory, Durham, R. A. Sampson.
1901, Sept. 17.

The 29-Year Eclipse-Cycle.

Ast. Journ. No. 504 contains an article by John N. Stockwell in which he calls attention to this cycle, which he states was known to Oppolzer, but has not been published before. In one lunation the line of conjunction of the Sun and Moon separates from the node by 30°·670; the most important multiples of this are as follows:—

47	lunations	produce a separation	of	1°·513	
88	„	„	„	179 ·003	
135	„	„	„	180 ·516	
223	„	„	„	359 ·519	The Saros.
358	„	„	„	180 ·035	

This shows that the period of 358 lunations (about 20 days short of 29 years) reproduces eclipses at sensibly the same distance from the node; hence we get series of eclipses of very much greater length than those in the Saros cycle. In the latter an eclipse continues for some 1300 years, being central for about $\frac{5}{8}$ of its whole career. But in the new cycle an eclipse is partial for about 200 returns, or 5800 years, then central for some 625 returns, or 18,100 years, finally partial again for 5800 years, making a total life-history of 29,700 years. So far, then, as the deduction of the New and Full Moons at which eclipses happen, and the approximate magnitudes of these eclipses, the cycle is an admirable one.

But it seems to me that in this article the writer has wholly overlooked one desideratum in an eclipse-cycle, which the Saros possesses, and which his cycle does not. The Saros, as is well known, reproduces the distances, diameters, and rates of motion of Sun and Moon with very considerable accuracy. A total eclipse is nearly always followed by a total one, and an annular one by an annular. Moreover, the motion of their tracks on the Earth's surface, both in longitude and latitude, is fairly steady and uniform. On the other hand, Mr. Stockwell's cycle does not give the smallest approximation to the size or distance of the Moon, the great total eclipse of the present year being preceded by annular eclipses in 1872 and 1843. Moreover, successive eclipses of this cycle happen at opposite nodes, their tracks generally lie on opposite sides of the equator, and there is no simple relation between their longitudes. Since it does not seem to me that an eclipse across Greenland can be said to bear any affinity to one 29 years later across the Antarctic Regions, I should be disposed to treat the new cycle as one of 58 years with a note that eclipses at the opposite node occur midway between successive members of the cycle.

Viewed in this way the cycle is formed by the simple superposition of the 47-lunation cycle on the triple Saros; both of these cycles had been already known and studied, so that the sole novelty lies in their combination.

The great point of the new cycle is that it enables us to say at once at what New and Full Moons eclipses took place right through the period covered by history; we can also form a rough idea of the latitude of the shadow-track, but none whatever of its longitude, so that we cannot say whether it would be visible in a given region, or whether it is annular or total. The Saros, on the other hand, gives with very considerable accuracy all the circumstances of an eclipse, when we have mapped out preceding members of the

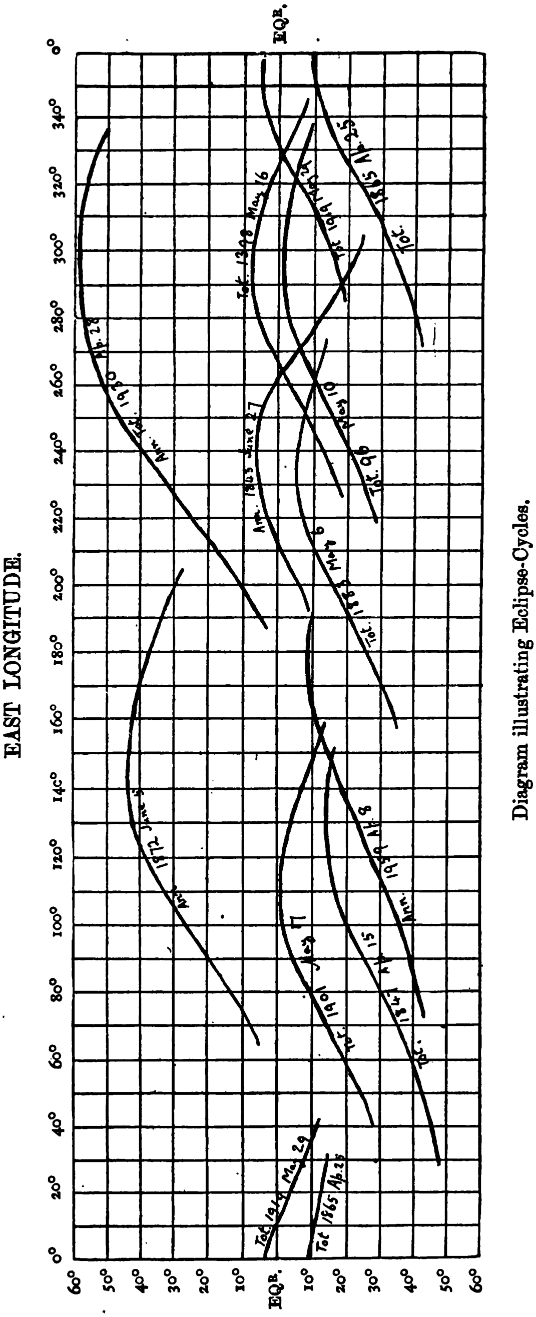

Diagram illustrating Eclipse-Cycles.

The eclipses of 1847, 1865, 1883, 1901, 1919 form a series in the Saros Cycle.
Those of 1843, 1872, 1901, 1930, 1959 illustrate the Stockwell Cycle.
Those of 1398, 1919 illustrate the 521-year Cycle (18 Stockwell Cycles).
Those of 96, 1901 illustrate the Megalosaros.

cycle; but of course it is available only for fairly short periods of time and is useless for the study of ancient eclipses. But we can here make use of the cycle of 22,325 lunations, or almost exactly 1805 years, discovered by M. Oppert. This cycle, for which I suggest the name Megalosaros, shares with the Saros the property of restoring the distances of Sun and Moon with almost perfect accuracy. Further we can with small labour compute an auxiliary table which gives the shift of the track in longitude, including the effect of secular acceleration; we can thus deduce all the circumstances of an eclipse with a considerable degree of accuracy, and can form an estimate whether it will be visible in a given locality. Of course for complete precision we must have recourse to the lunar tables; but it seems to me that in using Mr. Stockwell's cycle we have to do this before we can gain the roughest idea of the track of the shadow, and it is unquestionably a great gain to have a rough preliminary idea of the track, that we may avoid useless labour in computing the elements of eclipses that are invisible at the station we are considering.

I have constructed a diagram (p. 381) to illustrate the properties of these different cycles. The steady, stately westward sweep of the Saros tracks stands out in strong contrast to the capricious behaviour of the Stockwell tracks; further, the Saros tracks illustrated are all total, and the duration of totality varies slowly and steadily, while the Stockwell eclipses jump about at random from total to annular or *vice versâ*.

The period of 521 years (18 Stockwell cycles) gives somewhat closer approximations as regards duration of totality, though still much inferior to the Megalosaros. The latter gives surprisingly accurate results as regards duration of totality, and also as regards the latitude of the track, while the shift in longitude follows a fairly regular law and may readily be taken from an auxiliary table as stated above. This cycle is illustrated on the diagram by the eclipses of 96, 1901.

As regards practical utility, there does not appear any need for such cycles within the period covered by Oppolzer's Canon, which gives a close approximation to any eclipse that we desire to study. For epochs outside the Canon, my own preference would incline to the use of the Megalosaros, while allowing that Mr. Stockwell's results are full of interest, and present the sequence of eclipses in a new and suggestive light. A. C. D. Crommelin.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Democritus and Galileo on the Milky Way.

Gentlemen,—

Conjectures on the nature of the Milky Way before the invention of the telescope were numerous. (Of course I do not include amongst these the mythological absurdity to which it owes its name and the Greek and Latin equivalents.) But apparently the earliest to form a correct one was Democritus, the philosopher of Abdera, of whom very little is known, and of whose voluminous writings only a few fragments are extant. He is supposed to have lived, like Plato, to a great age, and to have died about the time when the victories of Epaminondas gave the Thebans their short-lived supremacy in the affairs of Greece. For his view of the nature of the Galaxy, we must have recourse to the treatise of Plutarch, 'De Placitis Philosophorum.' In the third book (c. 1) of that work he gives the ideas of different philosophers on the subject, amongst which he says:—

> *Δημόκριτος, πυλλῶν καὶ μικρῶν καὶ συνεχῶν ἀστέρων ουμφωτιζομένων ἀλλήλοις συναυγασμὸν, διὰ τὴν πύκνωσιν.*

The removal of this theory out of the region of conjecture took place when Galileo first directed a telescope to the starry heavens. His 'Sidenus Nuncius' was published early in 1610, about 1980 years after the death of Democritus. In this ever-memorable book he writes:—

"Quod tertio loco a nobis fuit observatum, est ipsiusmet LACTEI Circuli essentia, seu materies, quam Perspicilli beneficio adeo ad sensum licet intueri, ut et altercationes omnes, quæ per tot sæcula philosophos excruciarunt, ab oculata certitudine dirimantur, nosque a verbosis disputationibus liberemur. Est enim GALAXIA nihil aliud, quam innumerarum Stellarum coacervatim consitarum congeries: in quamcumque enim regionem illius Perspicillum dirigas, statim Stellarum ingens frequentia sese in conspectum profert, quarum complures satis magnæ ac valde conspicuæ videntur; sed exiguarum multitudo prorsus inexplorabilis est." (*Le Opere di Galileo Galilei, Edizione Nazionale*, vol. iii. p. 78.)

Probably Galileo thought that he had thus completely solved the problem of the Milky Way. But it is the wont of science to remove difficulties, and in doing so to create or bring into prominence others. Of the complex nature and structure of that wonderful zone he had no idea, and the labours of his successors have by no means exhausted scientific speculation with regard to it. Of course I have no intention here of touching on views which have been held and subsequently abandoned. But I should like before closing to allude to the very interesting pending problem, now that it is recognized that the irresolvable nebulæ have such a preference for the neighbourhood of the poles of the Milky Way, to endeavour to obtain some explanation of this.

Yours faithfully,

Blackheath, 1901, Sept. 6. W. T. LYNN.

Another Slow-pathed August Meteor.

GENTLEMEN,—

The remakable shooting-star described by Mr. Denning on p. 352 of this month's (September) number of the *Observatory* was not the only one among several meteors doubly observed during this year's August meteor-period whose real speed of motion was found to be of very decidedly notable and very certain slowness. Among the notes of meteor-paths recorded by several good observers in different parts of England, with copies of which I was kindly furnished by the present Director of the Meteor Section of the British Astronomical Association, Mr. W. E. Besley, a list of meteors mapped by Mr. J. H. Bridger at Farnborough, Hampshire, on August 10th, 1901, contained an observation, at $10^{h}\ 58^{m}$, of a 2nd magnitude shooting-star of which I was very pleased to have a duplicate description, as it was also noted here, at $10^{h}\ 58^{m}$, as a 2nd magnitude shooting-star of rather peculiar appearance. It passed here overhead from the south-eastward, from 1° north of γ Cygni to ½ (γ, ξ) Draconis, or from 305° +41° to 270° +54°, with very moderate speed for a very level-looking meteor-flight from such an eastern quarter, describing about 30° in 2, or in 2·1 seconds. It was nearly white, of 2nd magnitude, with a *coma*, or misty veil of white sparks nearly ½° wide in the first half of its course, leaving a faint white streak there for about one second; it then faded out for two or three degrees, and reappearing, described the rest of its course, with another slight intermittence of light on its way, as a nearly white, 4th–5th magnitude, rather hazy-headed meteor. At Farnborough (about 15½ miles from Slough, towards 25° west of south) its path was from 321° +53°½ to 270° +70½° (or from near π Cygni to near ϕ, ψ Draconis), also about 30° in length, reckoned, somewhat uncertainly, as occupying 1·4? second; and it was noted there as leaving a slight streak on its course and as directed, apparently, from some radiant in Aquarius.

The two paths' relative displacement in the sky agreed exactly in direction at their beginning and end points with the Slough-Farnborough base-line's direction; and the two nearly parallel tracks, prolonged far backwards, gradually approached and finally intersected each other (4° above the horizon, 41° S. of E.), at 356° − 20°, on the border-line, near g and β Ceti, between Aquarius and Cetus; probably an erratic centre place of the shower of 'β Cetids' at 3½° − 16° (Denning's General Catalogue, No. iv.), visible from the beginning of August to the middle of September. Being little more than 60° distant from the apex of the Earth's way, this radiant-point's theoretical meteor-speed, due to a parabolic orbit, should be about 30 miles per second. But the real speed of flight obtained by combining the above two observations, as the following computed heights and other measures showed, did not actually amount even to quite half of that theoretical-speed figure:—

Height at Beginning:—50 miles, over 3 miles north of Woking, Surrey.

Height at End:—48½ miles, over 4 miles S.S.E. from Watlington, Oxfordshire.

Length of Path, in nearly two seconds (*not less than* 1¾ sec.):—
24½ miles.
Real Velocity:—12 (and *not more than* 14) miles per second.

Since the double observation by Col. G. L. Tupman and Mr. H. Corder, on November 27th, 1877, of a small fireball's real speed, directed from between δ and *o* Draconis, of only 5 or 6, instead of 18 miles per second, with the exception, perhaps, of the 'γ Aquarid' (also doubly observed at Farnborough and Slough) on May 3rd, 1900, with a real velocity of about 23–27, instead of 40 miles per second, no instance immediately occurs to me of a shooting-star's observed velocity having been well proved, as in these two meteors' cases, to have been either certainly less than, or certainly not exceeding half, its parabolic theoretical velocity. It is rather a surprisingly strange and notable occurrence that the two present examples of such very slow-paced meteor-flights should have presented themselves together in a short list of only fifteen double meteor observations, obtained this year in little more than a week, between the 10th and the 21st of August.

I am, Gentlemen,
Yours most obediently,
A. S. HERSCHEL.

Slough, 1901, Sept. 23.

Conjunctions of Planets with Stars.

GENTLEMEN—

Several interesting events of this character will shortly occur, and the following are a few details.

In October Neptune will be closely W. of the variable star η Geminorum, the relative positions being:—

		h	m	s		°	′	″
η Geminorum	R.A.	6	8	57	Dec.	+22	32	8
Neptune at noon Oct. 8...		6	6	27		22	15	50
12...		6	6	24		22	15	43
16...		6	6	18		22	15	36

On Oct. 8 Neptune rises at about 8^h 50^m and will be visible during the remainder of the night.

Pallas was in opposition to the Sun on Sept. 7 (mag. 8), and may therefore be favourably observed during the present autumn.

On Oct. 9 the planet will be about 40′ S. and a little west of κ Aquarii (mag. 5·5):—

		h	m	s		°	′	″
κ Aquarii	R.A.	22	32	40	ec.	−4	44	17
Pallas at transit Oct. 9...		22	32	27		5	23	52

On October 13, 7^h, Venus will be placed 10′ N. of δ Scorpii (mag. 2·5). Venus sets at 6^h 31^m, or 1^h 21^m after the Sun.

Ceres will be in opposition to the Sun on Oct. 28 (mag. 7·4).

On October 13 the planet will be about 6′ S. of γ^2 Ceti (mag. 3·0):—

		h m s		° ′ ″
γ^2 Ceti........................	R.A.	2 38 12	Dec.	+2 49 20
Ceres at transit Oct. 13...		2 38 5		2 42 3

Uranus on October 15 will be less than ½° N. of 24 Ophiuchi (mag. 5·6):—

		h m s		° ′
24 Ophiuchi	R.A.	16 50 51	Dec.	−22 59
Uranus at noon Oct. 15...		16 50 54	··	22 32

On this date Uranus will set at about $7^h\ 16^m$, or $2^h\ 10^m$ after the Sun.

Juno on Oct. 23 will be some 6′ S. of 36 Sextantis (mag. 6·5):—

		h m s		° ′ ″
36 Sextantis	R.A.	10 40 6	Dec.	+3 0 18
Juno at noon Oct. 23 ...		10 40 18		2 54 0

Neptune on Nov. 24 will be about 3′ N. of the star Piazzi V. 338 (mag. 6½), the position of which will be

R.A. $6^h\ 3^m\ 36^s$ Dec. +22° 12′ 23″.

Some of these conjunctions may possibly enable amateurs to identify the fainter planets of which they may never have obtained glimpses before.

Yours faithfully,
W. F. Denning.

Bishopston, Bristol,
1901, Sept. 9.

The Total Solar Eclipse of 1832, *July* 27.

Gentlemen,—

In his letter on this eclipse (*Observatory*, vol. xix. p. 307) Mr. Johnson gives an account sent him by an old lady, whose name he does not mention, of her recollections of the phenomenon, she being at the time a young damsel "of the age of twelve years." He adds: "This is the first observation I have met with of this eclipse." It may perhaps be well to state that it was observed by Andrew Lang at Santa Cruz, which is about 16° to the east of Great Cayman, where the anonymous lady saw it. Mr. Lang's observations are recorded in *Ast. Nach.* No. 255 (vol. xi. col. 262). He noted the times of the beginning and end (from about $7^h\ 12^m$ to $9^h\ 34^m$ in the morning) and also a peculiar appearance at the end as if the Moon's limb hung for some seconds on the Sun's; but does not mention the corona or other phenomena. One would like to know whether the old lady's sketch was made at the time. The eclipse was observed in the afternoon as a partial one by Gambart at Marseilles (*Ast. Nach.* * No. 232, vol. x. col. 259), by Santini at Padua, by Messrs. Johnson and Armstrong at St. Helena, and by Don Sanchez Cerquero and Don Saturnino Montojo at San Fernando (*Memoirs of R.A.S.* vol. vi. p. 191).

Yours faithfully,
W. T. Lynn.

Blackheath, 1901, Sept. 2.

* In the index to the volume the date is erroneously given as July 2.

Daylight Meteors.

Gentlemen,—

I do not know if you will think the following worth publishing.

Yesterday, 18th September, having let my sidereal watch run down, I set it to sidereal time in the usual manner; to test its accuracy, I went into my observatory and brought Arcturus into the centre of the field. This was at $2^h\ 15^m$ p.m., in bright sunshine. While I was looking at the star, a shower of meteors, which nearly occupied the whole field of vision, passed from west to east, some nearly as bright as Arcturus. I then set my driving-clock in motion, and thinking that possibly there might be something wrong with my eyesight, I called my wife, and on looking through the telescope she at once saw the meteors, but the principal shower had then passed. However, they continued to pass across the field at the rate of about twelve a minute, sometimes more, while the star remained in the centre of the field, all coming from east to west. I continued watching until 3 p.m., when the meteors had nearly ceased to appear.

I have never heard of a shower of meteors being observed in bright sunlight, but I am perfectly convinced that I am not mistaken, especially as my wife, who has very good eyesight, saw them quite plainly. My telescope, a 7-inch Newtonian, is in perfect adjustment, a Kelner eyepiece was used, and my G.M.T. is correct to within a second. As I have mentioned, Arcturus remained in the centre of the field after the driving-clock was in motion.

Yours faithfully,

John Coles.

Lillooet, Liphook, Hants,
1901, Sept. 19.

[The above is printed with the full knowledge that there have been, before this, reports of meteors seen in daylight, which have not stood the test of examination. Mr. Coles's account is so circumstantial that it seems well to put the facts on record without expressing any opinion as to their interpretation.—Eds.]

OBSERVATORIES.

Cambridge University.—The Report of the work done at this Observatory during the year ending 1901, May 25, divides itself concisely under three heads. First, the Meridian-Circle work under Mr. Graham, which can be summed up by saying that 3773 meridian observations for both R.A. and Declination have been made during the year, the objects of observation being Nova Persei, Occultation Stars for Col. Tupman, Heliometer Comparison Stars and Zodiacal Stars for Sir David Gill. The reductions are all in a forward state. The second section of the Report deals with the Sheepshanks Photographic Equatorial under the care of

Mr. Hinks. The instrument has undergone some slight alteration since the last Report, as Dr. Common has supplied a new plane mirror in place of the one that he had previously ground, which proved to be slightly faulty. The instrument has been used during the year for observation of Eros and of Nova Persei. For the first object the Report states that the weather has not been wholly satisfactory, as on only seven nights was it possible to secure a series of photographs of sufficient length to give a satisfactory independent determination of the parallax. Nevertheless 596 exposures were made on the planet, 427 of which give measurable images. A fair series of plates has been obtained for determination of the relative photographic magnitude of the Nova and comparison stars.

The remainder of the Report treats of Mr. Newall's work with the telescope which bears his name. This has been used for observation on 93 nights in the course of the year. For the first ten months the large four-prism spectroscope was used for taking photographs of the spectra of some of the brightest stars. The spectrum of Capella has been specially photographed, and also that of α Persei, the latter having been closely studied to detect any signs of variability in the velocity of the star in the line of sight. "The results of earlier observations which pointed to a possible variation in a period of 4·2 days (circular orbit) or 16·8 days (elliptical orbit) have been communicated to the Royal Astronomical Society; but later observations, not yet published, show that neither of these periods will represent all the observations, and accordingly further material is being collected."

The spectrum of Nova Persei was photographed with the four-prism spectroscope up to March 9; on March 10 this instrument was dismounted and sent away to Sumatra for observation of the Solar Eclipse.

A new building is in course of construction on the north side of the Newall Dome, which will give much-needed space for experimental and laboratory work and for the accommodation of students.

WINDSOR, NEW SOUTH WALES.—Mr. Tebbutt's report of work for the year 1900 is on the same lines as that of former years. He begins by saying that various causes have combined to diminish the usual amount of work, but the decrease is apparently not more than might be expected, remembering that all the work is done by the proprietor, and advancing years must tell their tale. Local time was determined with the transit instrument on 83 nights, which is considerably fewer than in former years; and one point that may be noticed in this section is that several large changes of level-error happened after heavy rains. Such a change was noticed in Mr. Tebbutt's report of last year, and though a similar effect after rain has been noted in other places, the change at Windsor appears to be excessive, as may be seen from one case. On June 3 the level-error was $-4''$, on June 10 $-209''$; but

3·38 inches of rain had fallen in four days between these dates, an amount which does not fall to the lot of most places. Mr. Tebbutt has continued his series of observations of occultations of stars by the Moon. The total number of phases observed since 1864 now amounts to 1127. Search was made for two comets, Brorsen's and Giacobini's, but in neither case successfully. A telegram from Melbourne, giving an erroneous place, may have been partially the cause of failure in the latter case. The observations of double stars made at Windsor have been sent to the Royal Astronomical Society.

NOTES.

Comet Notes.—The following improved elements of the recent great comet (1901 I.) are by H. Thiele:—

T	1901 Apr. 24·2885 Berlin M.T.	
ω	203° 2′ 15″	1901·0.
☊	109 38 53	
i	131 4 49	
log q	9·38883	

Almost identical elements were given by Merfield.

Ephemeris for Berlin midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Oct. 3....	8 52 7	10 47	Oct. 15....	8 53 12	10 53
7....	8 52 45	10 48	19....	8 53 0	10 57
11....	8 53 4	10 50	23....	8 52 28	11 3

Its distance from the Earth will be about 3·4 in the middle of October, so that its visibility is very unlikely.

A considerable number of observations of Encke's Comet was secured in August and the beginning of September, after which it passed too near the Sun for observation. It is described as of the 10th magnitude, with traces of a tail. A. C. D. C.

Minor Planet Notes.—The following planets have been named:—

Number.	Name.	Number.	Name.	Number.	Name.
356	Liguria.	377	Campania.	405	Thia.
358	Apollonia.	378	Holmia.	409	Aspasia.
361	Bononia.	379	Huenna.	423	Diotima.
362	Havnia.	380	Fiducia.	424	Gratia.
363	Padua.	381	Myrrha.	425	Cornelia.
364	Isara.	382	Dodona.	432	Pythia.
365	Corduba.	388	Charybdis.	435	Ella.
367	Amicitia.	389	Industria.	436	Patricia.
370	Modestia.	390	Alma.	443	Photographica.
372	Palma.	397	Vienna.	446	Aeternitas.
373	Melusina.	402	Chloe.	449	Hamburga.
374	Burgundia.	403	Cyane.	451	Patientia.
375	Ursula.	404	Arsinoe.	454	Mathesis.
376	Geometria.				

Two new planets, GQ, GR, both of the 11th magnitude, were discovered by Carnera at Heidelberg on Aug. 17 and 23 respectively. Another, designated GS, proved not to be new, but identical with 453. GT was discovered by Wolf on Sept. 19, and GU by Carnera on Sept. 21.

The interesting planet 391 Ingeborg was photographed at Heidelberg on Aug. 8. This faint planet can only be observed at perihelion oppositions, of which the present is one; its distance from the Earth on Oct. 10 will be only 0·64. Before the discovery of Eros this was regarded as a very suitable planet for the deduction of the solar parallax. A. C. D. C.

Obituary.—Dr. Charles Meldrum, C.M.G., F.R.S., whose researches on the meteorology of Mauritius and the Indian Ocean were of fundamental importance, died on August 28th, at Edinburgh, where he had resided since his retirement from active duties in 1896. After passing through the University of Aberdeen, he entered the Bombay Education Department in 1846, and was transferred two years later to the scene of his life's work in Mauritius. At first a Professor of Mathematics in the Royal College, he early turned his attention to meteorology, and in 1851 assisted in founding the Meteorological Society of Mauritius, of which he was Secretary for many years. The Society published many papers of importance, and was, so far as we are aware, the only meteorological society that has ever flourished outside Europe and North America. Dr. Meldrum was appointed Government Meteorological Observer in 1862, and, after 1875, was Director of the Royal Alfred Observatory. For the last ten years of his life in Mauritius he was also a member of the Government Council of the colony.

Dr. Meldrum's name will always be associated with his work on the law of storms, especially the cyclones of the Indian Ocean, and with his early investigation of the relation of rainfall and other meteorological conditions to the sun-spot cycle.

Double Stars.—The observers at Lick still continue their searches for new double stars, and are to be congratulated on their success.

Professor Hussey publishes his third hundred in No. 494 of the *Astronomical Journal*, and from the following classification according to separation of the components it will be seen that they are of the same order as his previous discoveries:—

5	pairs	separated	0″·25 or less.
12	,,	,,	0·26 to 0″·50
20	,,	,,	0·51 to 1·00
25	,,	,,	1·01 to 2·00
38	,,	,,	2·01 to 5·00

Professor Aitken's Third Catalogue will be found in the 'Lick

Observatory Bulletin,' No. 3. There are 94 pairs, which, classified according to distance, are:—

3	pairs separated	0″·25 or less.
20	,, ,,	0·26 to 0″·50
21	,, ,,	0·51 to 1·00
29	,, ,,	1·01 to 2·00
20	,, ,,	2·01 to 5·00
1	,, ,,	over 5·00

Recent measures of ξ Bootis show a very marked divergence from the ephemeris computed by Dr. See in 1895. This has induced Professor Comstock to re-compute the orbit, using the measures up to 1901. We give both his and Dr. See's results:

	Comstock (1901).	See (1895).
P	172 years.	128·0 years.
T	1908	1903·9
e	0·49	0·72
a	5″·10	5″·56
☊	165°	10°·5
γ	35	52·3
λ	8	229·2

These orbits differ so much that for epoch 1904·5 See gives 125°·5 and 1″·03; Comstock 180° and 2″·6.

Greenwich measures for this year give 200° and 2″·8.

Messrs. Cogshall and Boothroyd, with the 24-inch Lowell refractor, have continued their re-measurement of the stars discovered by Dr. See, and publish a fine set of measures in Nos. 505–6 of the *Astronomical Journal.*

In the same issue Mr. Joshua Larson has re-computed the orbit of Σ 3062, and finds the following elements, which give unusually small residuals from the annual means:—

P	114·15 years.	☊	43°·65
T	1836·89	γ	47·48
e	0·444	λ	101·36
a	1″·492	μ	+3·1515

THE ECLIPSED SUN, MAY 18.—The picture which forms the frontispiece to this number, although it gives an excellent representation of the shape of the corona as seen by the naked eye and on the photographs, does not show the prominences which are such a striking feature of the camera pictures. One especially is remarkable. It will be remembered that shortly after the eclipse a large spot was seen near the eastern limb, which, at the time of eclipse, must have been just on the edge of the visible disk. In the photographs there appears a splendid prominence exactly at the position of this spot, thus giving direct evidence of the connection between the two phenomena.

A TELESCOPIC GUN-SIGHT.—The following is from the *English Mechanic* of August 16:—"There has been added to the exhibits in the Irish pavilion at the Glasgow Exhibition a new telescopic gun-sight, the invention of Sir Howard Grubb, F.R.S. This sight is on a totally new principle, there being neither back sight nor fore sight required. In taking aim you look through a small lens, when an image of a bright cross is thrown by an optical contrivance a long distance in front of the gun and in line with the barrel, which image forms the fore sight. For accurate aiming it is only necessary to superpose the cross on whatever object it is desired to hit, and then draw the trigger and the bullet does the rest. An expert gunsmith in Glasgow, after examining the invention on its being placed in the pavilion, expressed the opinion that it would lead to a complete revolution in the science of sighting guns and rifles."

THE LEEDS ASTRONOMICAL SOCIETY.—Mention has before been made in these pages of this Society and its excellent publication. The 'Journal and Transactions' for the year 1900 appears especially interesting. We read with great profit the article by Mr. W. D. Barbour contained therein, on "Traditions of Creation," which gives an excellent scientific interpretation of the biblical narrative. Dr. Pocklington contributes an excellent summary of our present knowledge of "Magnetism in the Solar System"; and Miss Barbour a paper on "Astrology." The volume opens with a paper by the President, Mr. Whitmell, "The Planet Venus as a View point," which seems to be one of a series that he is writing, and whose substance may be inferred from the title. These are ingenious, but they always seem to us only a step or two above the magazine articles which tell us how much the railway fare to the Sun would be and such like. Mr. H. J. Townshend is the President for the current year.

THE WEATHER IN SEPTEMBER.—The month has been very fine and sunny, with a few warm days; but on the whole the monthly mean temperature has been about the average value. The highest temperature occurred on the 8th, and was 76°·9, and there were five other days on which the temperature exceeded 70°. The lowest temperature was 39°·6 on the 15th. The recorded total of bright sunshine in the month amounted to 121·7 hours, being about 32 per cent. of the possible duration. The rainfall amounted to 1·35 inch, derived from falls on six days only. This amount is less than the average for the preceding 60 years by 0·83 inch. These remarks apply to the London district, but there appears to have been a general deficiency of rainfall throughout the country.

WE are informed that Dr. Isaac Roberts, F.R.S., the well-known astronomer, is shortly to marry Mdlle. Dorothea Klumpké, late of the Paris Observatory, to both of whom we wish all happiness.

FROM AN OXFORD NOTE-BOOK.

THE British Association met this year at Glasgow. The President (A. W. Rücker) gave a thoroughly able and lucid defence of "atoms and the ether," insisting that they were not "mere figments of the scientific imagination"; nor was it the only necessary alternative that "a mechanical theory of the atoms and of the ether, now confessedly imperfect, would, if it could be perfected, give us a full and clear representation of the underlying realities." The *Times* complained that "the problems glanced at were very much beyond the average competence of amateurs," and wrote unsympathetically about the Address in consequence; but both those who heard the Address and those who read it afterwards will hesitate to accept this judgment. In Section A, Major MacMahon declared that the apparent isolation of certain departments of pure mathematics was only temporary or fictitious. "I do not believe," he said, "in any branch of science or subject of scientific work being destitute of connection with other branches. If it appears to be so, it is especially marked out for investigation by the very unity of science. . . Now a department of pure mathematics which appeared to be somewhat in this forlorn condition a few years ago was that which included problems of the nature of the magic square of the ancients." And he went on to explain how this apparent isolation had recently been removed.

But perhaps some of the earlier paragraphs in his Address were more appreciated by the audience—those, for instance, in which he sketched the history of the old Spitalfields Mathematical Society, founded by the Spitalfields weavers in 1717, and absorbed into the Royal Astronomical Society in 1843. Some of the rules of this Society were very quaint. "Every member present was entitled to a pint of beer at the common expense; and, further, every five members were entitled to call for a quart for consumption at the meeting. It was found necessary to introduce a rule fining members sixpence for letting off fireworks in the place of meeting." The members dined together in London on the second Friday in January, "in commemoration of the birth of Sir Isaac Newton"; and it may be remarked that this day is still chosen by the Royal Astronomical Society Club for their Annual Meeting.

THERE was not a great deal of astronomy at the Meeting: but one morning was filled up with papers on various astronomical topics:—a possible planet beyond Neptune, micrometers, motion of faculæ, &c. Later in the week Lord Kelvin attracted a large audience by dilating on the possible amount of gravitational matter in our universe, the gauge made use of being the magnitude of the velocities of stars. Existing proper motions (in miles per second) indicate that there is the equivalent of between 100 and 1000 million Suns in a sphere the surface of which has a stellar parallax of $0''\cdot003$: this was the main result, if I remember aright. The meteorologists had several meetings, including a breakfast. And a good many mathematicians turned up in Section L (the new section for Education) to hear Prof. Perry's vigorous attack on the present teaching of mathematics. He said it was taught as though every boy was going to be a pure mathematician, which was hard on the average boy. "We are mentally destroying 10,000 Toms, Dicks, and Harrys for the sake of producing one man fit to be a mathematical master of a second-rate school." He suggested as a remedy that we should take a system which he had tried for many years—a short course wherein useful things were taught and useless (especially Euclid) omitted. Of course there were many feathers ruffled. It was replied to Prof. Perry that he wanted to train every boy as though he were going to be an engineer: and a good many other things were said. We had quite a successful discussion, under the presidency of Sir John Gorst, which lasted all the morning. And I will set down here how the meeting terminated, for it impressed me deeply. Sir John Gorst summed up at the end in a manner quite in keeping with what had gone before. Then, after pausing a moment, he said there was another matter to refer to. Since we had entered the room we had heard of the death of President McKinley. It was too true: the news had been whispered during the debate, and though it caused no interruption at the time, the deep feeling which it excited was shown by the way in which all rose to their feet when these words came from the Chair. They remained standing while Sir John Gorst constrasted in a few eloquent words the loss of America with that which we had recently suffered, and referred to the sympathy we had received, which we could now return; and then the meeting, which had been until a few minutes before so animated, silently dispersed.

But if the number of astronomical papers was not great, astronomers have every reason to remember their visit to Glasgow with satisfaction. In the first place, there was a very pleasant gathering at the hospitable board of Professor and Mrs. Becker; and it may be added that Glasgow generally is a most friendly and hospitable city. Secondly, a visit to the Observatory on the Sunday afternoon provided much material of a suggestive kind. One sees on such occasions things which may have been mentioned in print already, but do not impress one nearly so much until actually seen at work. For instance, the glass rods for illuminating the microscopes. Prof. Becker does not want the lamps themselves near the circle because of the heating effect, but he wants their light conveyed thither with as little diminution as possible. So he takes a solid glass rod and lets the light shine in at one end; once inside it cannot get out, owing to the well-known phenomena of internal reflection; and so it travels to the other end and is delivered at the circle where required, much as a jet of water is conveyed by an india-rubber tube. The glass rod can go round corners even—altogether a beautiful device. Of course the absorption of the glass is a factor which must not be forgotten; but there is no doubt the plan works beautifully within its proper limits. Some of Professor Becker's ingenuities have been adopted elsewhere and are now tolerably familiar—*e. g.* the small prism cemented to the object-glass for illumination of the field, and his thermopile for charging accumulators. He is strong on the point that oil should not be used for bearings, but vaseline, and that you cannot put too much vaseline on. An obviously useful diagram (which, however, I have seldom seen elsewhere) is one showing the altitude of the Sun, and therefore the darkness or twilight, at any hour of the night throughout the year. We were glad to hear that some of Prof. Becker's work is on the point of being published—work, however, that was done at Edinburgh and will appear in the Edinburgh publications. He has observed a small star 6′ from the Pole for a number of years in all hour-angles, which should provide some material for discussion of a novel kind, and is still continuing these observations, though this is only one of several pieces of work.

Being away from references, I cannot recall exactly what was said on the subject to which the following letter refers; but I remember that at the time it seemed quite possible that the writer had been the first to see the great comet of last spring, and regret was expressed that he had not been a little more precise in his information, so that the credit might be assigned to him if really due. The following letter makes it abundantly clear how impossible it was to be precise, and, moreover, gives such a vivid picture of the surroundings that I can scarcely regret a misjudgment which has elicited so interesting a reply:—

Orange River Station,
South Africa.
1901 Aug. 12.

Dear Sir,—

I have seen my note on the great comet 1901 in the July number of the *Observatory*; also Mr. Boys has forwarded your letter on the subject. I must disclaim being the *discoverer* of the comet, as it was certainly April 24 before I saw it, and I only looked for it because a patrol of Boers came and told me they had seen it the day before. I quite appreciate your saying that I should be "more precise and early in my information," but when you hear the circumstances under which I was *observing* at the time you will say I didn't do so badly. Our watches and instruments had been packed up and buried since October 1900, as we were surrounded by Boers and supposed to be cut off from any outside communication. I was always at work about the farm, sowing, ploughing, driving the irrigation machinery, or herding stock, so that I began to have a very hazy idea as to what the actual date was. When I posted that letter to Mr. Boys I had to watch the Boers safe off the farm, saddle up at night, swim the Orange River (200 yards of water running 4 knots), and ride 20 miles to Orange River Station. It was there at the post office that I discovered it was the 27th April, and it was 3 days before this that I saw the comet first.

It was funny with what a solemn face the Boers reported the comet to me, as though they meant the comet had come without orders—that I was supposed to understand such things and they would hold me responsible for its good behaviour!

Yours very sincerely,
T. B. Parkinson.

Three or four months ago I gave in these Notes the original story about Sydney Smith and the North Pole, as it is sometimes of interest to recur to the original of a story which is often repeated and is apt to be modified in the repetition. The story is apparently new in Germany and is recounted with great gusto by a German contemporary, who incidentally gives a striking example of the way in which modifications are introduced. The name Sydney Smith was unfamiliar—*ergo* it must be a disguise for some better known name. The story being in an astronomical magazine it should be an astronomer of note. Smith, Smith! what name is there of an astronomer like that? Why Smyth, of course—Admiral Smyth, who compiled the 'Celestial Cycle'! And so our contemporary, to simplify matters, relates the story of Admiral Smyth, whom he calls indifferently Admiral Smyth or Admiral Sydney Smith. It may be added that another paragraph, from the pen of G. B. S., is also reproduced with apparent appreciation; but the subtlety of G. B. S.'s humour has led to a terrible misapprehension; he is briefly designated "Einen andern Narren, Namens Shaw"!!

The Perseid meteor shower has this year yielded some surprising observations, which conclusively prove either abnormal features in the meteors or some anomalous traits in the observers. In the *English Mechanic* we have had described a new kind of meteors, nebulous bodies which return along the paths of previous meteors. "A swift Perseid would dart along, and then shortly afterwards a nebulous body would travel in the reversed direction along the path, as though the luminous residue from the exhausted meteor returned to the radiant." One would be reminded of the manner in which the residual stick from an exhausted rocket returns to the earth if only the stick were nebulous and luminous, which it conspicuously is not. The same observer also saw meteor showers from points in the sky corresponding to the Novæ of 1572 (Cassiopeiæ), 1876 (Cygni), and 1892 (Aurigæ), but no confirmatory observations elsewhere are forthcoming. Another observer had the good fortune to see 149 first-class fireballs on August 13 between $10^h\ 30^m$ and $13^h\ 30^m$; and notes as an important fact that their motion was not away from, but *towards* Perseus. He admits a difficulty in noting their directions accurately owing to their great velocities; but for position-angles to be uncertain by 180° is a large order, almost, as large as the 149 first-class fireballs.

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EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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The McClean Gift to the Cape Observatory.

On Thursday, September 26, His Excellency the Hon. Sir Hely Hutchinson, in the presence of a distinguished company to the number of fifty or sixty, unveiled the Inscription Stone of the new Telescope of the Cape Observatory. The following account of the proceedings, slightly abbreviated, is from the *Cape Argus* of 1901, October 2:—

Sir David Gill. May it Please Your Excellency,—The ceremony which I am about to ask Your Excellency to perform is one regarding which some explanation seems to be necessary. We all understand the significance of the laying of a foundation stone or the formal opening of a new building, but it is evident that the function of to-day represents neither the one nor the other. Your Excellency will therefore perhaps permit me to enter into a short account of the origin and history of this great telescope and of the observatory under the dome of which we are now assembled. If in so doing I appear at first somewhat discursive, I trust to be forgiven on account of the interest of the subject and of the occasion. Until about 40 years ago the science of astronomy concerned itself chiefly with the positions of celestial objects. It occupied itself with the observation of their apparent places in the sky, tracing the origin of their motions, and finally computing and predicting these motions for all past and future time. It has measured not only the dimensions and determined the elements of our planetary system, but it has also made no small progress towards a knowledge of the dimensions of the Sidereal System and of the amount and direction of our Sun's motion through space. It has catalogued the stars to a high order of magnitude, and determined many facts as to their distribution in space. There is no subject to which higher genius has applied itself than that of unravelling the celestial motions and the laws which govern them. Thus the old astronomy, from the difficulties of her task, the beauty and precision of her methods, and the proved accuracy of her predictions, has earned for herself the acknowledged position of queen of the sciences. But her task is by no means ended, for the so-called old astronomy still provides, and for ever will provide, a boundless field for research and for exercise of the highest efforts of the human intellect. Sixty years ago no one believed it possible that astronomy could embrace the study of the constitution as well as the motions of celestial objects. It is true that the speculations of Laplace seemed so well based, and to fit so well with known facts and scientific possibilities, as to afford the belief that the Sun and planets had been evolved from common primordial matter. But Laplace's views could only be regarded in the light of a hypothesis; they were not capable of that proof which is necessary to raise speculation, however plausible, to the level of scientific truth. Comte, in his 'Cours de Philosophie Positive,' expressed the opinion of his time thus:—"We may speculate with some hope of success on the formation of the Solar System of which we form a part, for it presents to us numerous perfectly well-known phenomena, susceptible perhaps of giving proof of its true immediate origin. But what, on the other hand, could possibly form a rational basis for our conjectures on the formation of other suns? How confirm or disprove by the evidence of phenomena any cosmogonical hypothesis when no phenomena of such a kind are known, nor, doubtless, are even knowable?" In other words, the philosophic dictum of sixty years ago was that the chemical constitution of other systems than our own is a subject which, from the nature of things, must be regarded as unknowable. But the discovery of the lines in the Solar Spectrum by Fraunhofer, the interpretation of the meaning of these lines by Kirchhoff and Bunsen, and the application of the spectroscope to other celestial objects has upset that philosophic conception of the unknowable, and given to us the new astronomy. This new astronomy does not deal so much with the position as with the constitution of celestial objects. Its aim is not so much to answer the question where is such a star, but what it is, what can we find out about its chemical constitution, and the chemical history of its development?

But with all this distinctive difference between the new and the old astronomy it is impossible to divorce the one from the other. There is perhaps no finer illustration of the co-relation of the physical sciences than is to be found in the outcome of this new development of astronomy. The old astronomy required the combined efforts of the optician, the mechanician, the engineer, the observer, and the mathematician for its pursuit, the new astronomy adds those of the physicist and the chemist, and we are every day finding out not only how each and all of these branches of science contribute to the advancement of astronomy in general, but also how their common application to astronomy has contributed to the advancement of those separate sciences. I may perhaps be permitted to dwell briefly on this most interesting subject.

The observations of Sir William Huggins, one of the first workers in this field, showed that many well-condensed nebulæ, as well as vast patches of nebulous light in the sky gave only bright lines in the spectroscope—lines which proved that such nebulæ were not stars at all, but inchoate masses of luminous gas. Evidence upon evidence has accumulated to show that such nebulæ consist of the matter out of which stars (*i.e.*, suns) have been, and are being, evolved. The different types of star-spectra form such a complete and gradual sequence (from a simple spectrum resembling those of nebulæ, onwards through types of gradually increasing complexity) as to suggest that we have before us, written in the cryptograms of these spectra, the complete story of the evolution of suns from the inchoate nebulæ onwards to the most active sun (like our own) and then downward to the almost heatless and invisible ball. The period during which human life—nay, even life of every kind—has existed on our globe is probably too short to afford observational proof of such a cycle of changes in any particular star, but the fact of such evolution, with the evidence before us, can hardly be doubted. I most fully believe that when we have farther studied the modifications of terrestrial spectra, under sufficiently varied conditions of temperature, pressure, and environment, our certainty of the fact will be greatly increased. But in this study we must also have regard to the spectra of the stars themselves. The stars are the crucibles of the Creator, where we see matter under conditions of temperature and pressure and environment the variety of which we can hardly hope to emulate in our laboratories, and on a scale of magnitude beside which the scale of our greatest experiment is less than that of the drop to the ocean. I believe we must look to the new astronomy for aid in the solution of many great chemical problems.

The astronomer of the new school has to thank the physicist and the chemist for the foundation of his science, but the time is coming—we almost see it now—when the astronomer will repay the debt by wide-reaching contributions to the very fundamenta of chemical science. Thirty years ago there was first observed, in the spectrum of the Sun's chromosphere, a very remarkable bright yellow line, near the position of the well-known D lines of sodium. So distinctive was this line, and so certainly not due to any known terrestrial substance, that it was called the helium line. In 1894 Lord Rayleigh, who was engaged in determining the densities of the principal gases, found what was then to him an inexplicable difference between the weight of a volume of nitrogen prepared from atmospheric air and the weight of the same volume of nitrogen prepared from ammonia or by other chemical means. Repeated experiment showed that the weight of the constant volume of atmospheric nitrogen was about 1·200 greater than that of the chemically prepared gas. After exhausting all means of testing the purity of the chemically prepared nitrogen, Lord Rayleigh and Professor Ramsay, in January 1895, finally traced the cause of their perplexity to a hitherto unknown gas present in our atmosphere which they named Argon. Here was a great chemical discovery due to the co-operation of the physicist and the chemist. On the publication of this paper Mr. Meirs, of the British Museum, directed Ramsay's attention to a paper by Hillebrand, in which the author had found that the mineral uranite contained nitrogen; and Ramsay naturally was desirous of examining every source of nitrogen. Accordingly he boiled clevite—a uranite of lead containing rare earths—with weak sulphuric acid, and after collecting the evolved gas he found that its spectrum gave not only the now known argon lines but also new lines, one of which, to Ramsay's intense surprise and delight, absolutely coincided with the helium line, which had been known for twenty-six years in the spectrum of the solar chromosphere. Of course, as soon as helium was prepared its spectrum was thoroughly studied, and then Lockyer and McClean were quick to show that many of the lines, which occurred in the spectra of a large class of stars, were due to this same helium. Here was another chemical discovery in which the astronomer and the chemist were mutually helpful—a discovery also that is yet destined to throw much light on the evolution of stars.

One more illustration and I am done. The study of the phenomena of light has compelled the conviction that light is the result of vibrations or waves in ether, as sound is the result of vibrations in air, and that just as slow and rapid vibrations of air produce respectively low and high pitched notes, so do slow and rapid vibrations of ether produce red and yellow or blue and violet light respectively. Doppler pointed out in 1841 the possibility of determining the velocity of approach or recession of the star, provided that the velocity of the star's motion has a measurable relation to the velocity of light; but it was not until Huggins had begun stellar spectroscopy that about 1865 his attention was directed by Clerk Maxwell to this possibility of the new astronomy, and that in 1866 he made the first attempts to determine motions in the line of sight. Such a task was, of course, impossible until the lines of the star-spectra had been identified with those of known terrestrial substances, just as it would be impossible for an observer to determine the velocity of a railway train at any moment by means of the note of the whistle that reached his ear, unless the observer also had a tuning-fork emitting the same note as the whistle of the engine when at rest. But Huggins had already identified many star-lines with those of terrestrial spectra, and, so far, was in a position to attempt the task. He showed in 1866 that such work was possible, but it required the application of photography (first used for this purpose by Vogel) and exhaustive study of the theory of the spectroscope, and the greatest refinement in its construction and its use, to give the new engine of research the requisite reliability. These preliminary difficulties are now overcome, and daily results of the greatest importance are being added to our storehouse of knowledge. It would occupy too long were I to enter on the numerous problems to which this branch of the new astronomy is applicable, but it will be evident how great an advantage to astronomy must be this new power to determine not, as formerly, only angular velocity to right angles to the line of sight, but the actual linear velocity of motion in the line of sight itself. These examples, which I fear I have quoted at too great length, enable me to explain in a few words the full significance of the ceremony which we have assembled to-day to witness. Until the year 1894 there existed, neither at the Cape nor in any Observatory in the Southern Hemisphere, any adequate equipment for pursuit of the new astronomy, nor was there apparently much hope of the need being supplied. For forty years the new astronomy had been vigorously prosecuted in the Northern Hemisphere, the first great harvest of results obtainable with moderate means had been reaped, and great establishments were founded for research in the new fields of work. It thus became obvious that if anything was to be done to equalize the possibilities of research in these new fields in both hemispheres, no small outlay would be required.

On my appointment as H.M. Astronomer in 1879, Mr. Newall, who then possessed the largest telescope in England, offered the loan of it to prosecute research in the new astronomy at the Cape, but it was considered by the authorities at home that the cost of its transport and the erection of a suitable building and dome could not be entertained unless the telescope might remain permanently the property of the Observatory. Mr. Newall had good reason for limiting his offer to loan; for his son, then a young man, gave promise of scientific tastes, is now using that instrument at Cambridge, and obtaining with it some of the most refined spectroscopic results that have yet been secured in England. The busy years rolled on, and I had almost resigned myself to the idea that, during the period of my directorate at least, the Royal Observatory at the Cape must limit itself to the pursuit of the old astronomy, for which purpose it was well equipped. But in 1894 arrived a letter from Mr. Frank McClean, offering to present, for the use in the Southern Hemisphere, and preferably to the Cape, a telescope and observatory, the specification for which corresponds with the instrument now before us, and the building in which we are now assembled. Mr. McClean further stated that the optical part of the instrument had been for some time under construction by Sir Howard Grubb, of Dublin, and the whole would probably be completed before the end of 1896. The new instrument was also to be fitted with object-glass, prism-spectroscopes, &c., so that, upon the completion of all, the Cape Observatory might enter on the pursuit of the new astronomy with every advantage possible in the way of equipment. Here was indeed a revival of hopes almost dead, of ambitions almost abandoned. The value of the gift was, if possible, enhanced by the fact that Mr. McClean is himself a distinguished worker in Astrophysics. We had seen his splendid photographs of terrestrial spectra, we knew something, but not all, of the great work on which he was then engaged, viz., of obtaining incomparable spectra of all the stars to the 3½ order of magnitude, and we felt that his gift was due solely to a clear and well founded perception of the needs of science, and of an earnest and helpful desire to fulfil them. The Lords Commissioners of the Admiralty accepted, with warm appreciation of Mr. McClean's generosity, the offer of this splendid instrument, and expressed the view that its possession would greatly increase the utility of the Cape Observatory, and might be expected to result in considerable advancement to science. The year 1896 saw the observatory building ready for reception of the telescope and the dome erected. In the following year Mr. McClean visited the Cape, attached his object-glass prisms to our photographic telescope, and was thus enabled to complete that remaining portion of his spectroscopic survey of the whole heavens which could not be completed from his own observatory in Kent. His work at the Cape was also memorable by his discovery of the existence of oxygen in the spectra of a certain class of stars, and for this discovery and his spectroscopic labours generally, he was awarded the Gold Medal of the Royal Astronomical Society of London in 1899. With the fullest expectation that the instrument would be erected during 1897 Mr. McClean had ordered the inscription stone which Your Excellency is about to uncover to be cut. There have been disappointing delays, but it it will be found that the stone bears the inscription "The Victoria Telescope," and the date 1897, the year when the donor intended that the telescope should be completed and. this ceremony performed. It is named the Victoria Telescope in honour of the great and good Queen whose jubilee it was intended to celebrate, and to whose beloved memory only it must now stand. I venture to hope that it will long stand to honour that memory and to fulfil, by useful work, the noble intentions of its large-minded donor.

His Excellency the Governor, in a happy little speech, said that he did not think that Sir David Gill need have offered any apology for the length of his address, for he was sure they had all found it a most interesting one. (Hear, hear.)

The whole company then proceeded to the outside of the building, where His Excellency removed the Union Jack which covered the inscription stone. The inscription was simply as follows :—

1897 : THE VICTORIA TELESCOPE.
THE GIFT OF FRANK McCLEAN,
OF RUSTHALL, KENT.
DAVID GILL, H.M. ASTRONOMER.

While assembled round the stone the gathering was twice photographed by the photographer to the Observatory. Sir David Gill then called for three cheers, first for the donor of the telescope, Mr. McClean, and then for His Excellency the Governor, both of which were given with cordial goodwill. The guests were then invited to partake of refreshments, under Sir David Gill's roof, and the proceedings terminated.

On the Clustering of Gravitational Matter in any Part of the Universe.

In the Mathematical and Physical Science Section of the British Association, Lord Kelvin delivered the following discourse on "The Absolute Amount of Gravitational Matter in any large Volume of Interstellar Space."

According to the usual ideas of universal gravitation, gravitational matter would be *all* matter, nor is there any matter which is not subject to the law of gravitation. I think I may say with absolute decision that there is. We are all convinced, with our President, that ether is matter, but we are forced to say that the properties of molar matter are not to be looked for in ether as generally known to us by action resulting from force between atoms and matter, ether and ether, and atoms of matter and ether. Here I am illogical, said Lord Kelvin, when I say "between matter and ether," as if ether were not matter. It is to avoid an illogical phraseology that I use the title "gravitational matter." Many years ago I gave strong reason to feel certain that ether was outside the law of gravitation. We need not absolutely exclude, as an idea, the possibility of there being a portion of space occupied by ether beyond which there is absolute vacuum—no ether and no matter. It must be admitted that that is something that one could think of; but I do not believe any living scientific man considered it in the slightest degree probable that there was space surrounding our universe beyond which there is no ether and no matter. Well, if ether went through all space, then it is certain that ether cannot be subject to the law of mutual gravitation between its parts, because if it were subject to mutual attraction between its parts its equilibrium would be unstable, unless it were infinitely incompressible. But here again I am reminded of the critical character of the ground on which we stand in speaking of or giving very definite propositions beyond what we see or feel by experiment. I am afraid I must here express a view different from that which Prof. Rücker announced in his address, when he said that continuity of matter implied absolute resistance to condensation. We have no right to bar condensation as a property of ether. While admitting ether not to have any atomic structure, it is postulated as a material which performed functions of which we know something, and which may have properties allowing it to perform other functions of which we are not yet cognizant. If we consider ether to be matter, we postulate that it has rigidity enough for the vibration of light, but we have no right to say that it is absolutely incompressible. We must admit that sufficiently great pressure all round could condense the ether in a given space, allowing the ether in surrounding space to come in towards the ideal shrinking surface. When I say that ether may be outside the law of gravitation, I assume that it is not infinitely incompressible. I admit that if it were infinitely incompressible, then it may be subject to the law of gravitation between its parts; but to my mind it seems infinitely improbable that ether is infinitely incompressible, and it appears more consistent with the analogies of the known properties of molar matter, which should be their guides, to suppose that ether has not the quality of exerting an infinitely greater force against compressing action of gravitation. Hence if we assume that it extends through all space, ether must be outside the law of gravitation, that is to say, truly imponderable. I remember, continued his Lordship, the contempt and self-complacent compassion with which sixty years ago most of the teachers of that time—and I myself, I am afraid—looked upon the ideas of the elderly people who went before them, who spoke of "the imponderables." I fear that in this, as in a great many other things in science, we have to hark back to the dark ages of fifty, sixty, or a hundred years ago, and that we must admit there is something which we cannot refuse to call matter, but which is not subject to the Newtonian law of gravitation. That the Sun, stars, planets, and meteoric stones are all of them ponderable matter is true, but the title of my paper implies that there is something else. Ether is not any part of the subject of this paper; what I am dealing with is gravitational matter, ponderable matter. Ether we relegate not to a limbo of imponderables, but to distinct species of matter which has inertia, rigidity, elasticity, compressibility, but not heaviness. In a paper I have already published I have given strong reasons for limiting to a definite amount the quantity of matter in space known to astronomers. I can scarcely avoid using the word "universe," but I mean our universe, which may be a very small affair after all, occupying a very small portion of all the space in which there is ponderable matter.

Supposing a sphere of radius $3{\cdot}09 . 10^{16}$ kilometres (being the distance at which a star must be to have parallax $0''{\cdot}001$) to have within it, uniformly distributed through it, a quantity of matter equal to one thousand million times the Sun's mass, the velocity acquired by a body placed originally at rest at the surface would, in five million years, be about 20 kilometres per second, and in twenty-five million years would be 108 kilometres per second (if the acceleration remained sensibly constant for so long a time). Hence if the thousand million suns had been at rest twenty-five million years ago, uniformly distributed throughout the supposed sphere, many of them would now have velocities of twenty or thirty kilometres per second, while some would have less and some probably greater velocities than 108 kilometres per second; or, if they had been given at rest thousands of million years ago so distributed that now they were equally spaced throughout the supposed sphere, their mean velocity would now be about 50 kilometres per second (*Phil. Mag.*, August 1901, pp. 169, 170). This is not unlike the measured velocities of stars, and hence it seems probable that there might be as much matter as one thousand million suns within the distance $3{\cdot}09.10^{16}$ kilometres. The same reasoning shows that ten thousand million suns in the same sphere would produce velocities far greater than the known star velocities, and hence there is probably much less than ten thousand million times the Sun's mass in the sphere considered. A general theorem discovered by Green seventy-three years ago regarding force at a surface of any shape, due to matter (gravitational, or ideal electric, or ideal magnetic) acting according to the Newtonian law of the inverse square of the distance, shows that a non-uniform distribution of the same total quantity of matter would give greater velocities than would the uniform distribution. Hence we cannot, by any non-uniform distribution of matter within the supposed sphere of $3{\cdot}09.10^{16}$ kilometres radius, escape from the conclusion limiting the total amount of the matter within it to something like one thousand million times the Sun's mass.

Lord Kelvin then went on to compare the sunlight with the light from the thousand million stars, each being supposed to be of the same size and brightness as our Sun; and stated that the ratio of the apparent brightness of the sun-lit sky to the brightness of our Sun's disc would be $3{\cdot}87.10^{-13}$. This ratio (*Phil. Mag.*, August 1901, p. 175) varies directly with the radius of the containing sphere, the number of equal globes per equal volume being supposed constant; and hence to make the sum of the apparent area of discs $3{\cdot}87$ per cent. of the whole sky, the radius must be $3{\cdot}09.10^{27}$ kilometres. With this radius light would take $3\frac{1}{4}.10^{14}$ years to travel from the outlying stars to the centre. Irrefragable dynamics proves that the life of our Sun as a luminary is probably between 50 and 100 million years; but to be liberal, suppose each of our stars to have a life of 100 million years as a luminary, and it is found that the time taken by light to travel from the outlying stars to the centre of the sphere is three and a quarter million times the life of a star. Hence it follows that to make the whole sky aglow with the light of all the stars at the same time the commencements of the stars must be timed earlier and earlier for the more and more distant ones, so that the time of the arrival of the light of every one of them at the Earth may fall within the durations of the lights of all the others at the Earth. His supposition as to uniform density is quite arbitrary; but nevertheless he thought it highly improbable that there could be enough stars (bright or dark) to make a total of star-disc-area more than 10^{-12} or 10^{-11} of the whole sky.

To help to understand the density of the supposed distribution of 1000 million suns in a sphere of $3{\cdot}09.10^{16}$ kilometres radius, imagine them arranged exactly in cubic order, and the volume per sun is found to be $123{\cdot}5.10^{39}$ cubic kilometres, and the distance from one star to any one of its six nearest neighbours would be $4{\cdot}98.10^{13}$ kilometres. The sun seen at this distance would probably be seen as a star of between the first and second magnitude; but supposing our 1000 million suns to be all of such brightness as to be stars of the first magnitude at distance corresponding to parallax $1''{\cdot}0$, the brightness at distance $3{\cdot}09.10^{16}$ kilometres would be one one-millionth of this; and the most distant of our stars would be seen through powerful telescopes as stars of the sixteenth magnitude. Newcomb estimated from 30 to 50 million as the number of stars visible in modern telescopes. Young estimated at 100 million the number visible through the Lick telescope. This larger estimate is only one-tenth of our assumed thousand million masses equal to the sun, of which, however, nine hundred million might be either non-luminous, or, though luminous, too distant to be seen by us at their actual distances from the Earth. Remark also that it is only for facility of counting that we have reckoned our universe as a thousand million suns; and that the meaning of our reckoning is that the total amount of matter within a sphere of $3{\cdot}09.10^{16}$ kilometres radius is a thousand million times the Sun's mass. The Sun's mass is $1{\cdot}99.10^{27}$ metric tons, or $1{\cdot}99{\cdot}10^{33}$ grammes. Hence our reckoning of our supposed spherical universe is that the ponderable part of it amounts to $1{\cdot}99.10^{42}$ grammes, or that its average density is $1{\cdot}61.10^{-23}$ of the density of water.

Lord Kelvin returned to the question of sum of apparent areas, the ratio of which to 4π, the total apparent area of the sky viewed in all directions, is given by the formula (*Phil. Mag.*, August 1901, p. 175) $a = \frac{3N}{4}\left(\frac{a}{r}\right)^2$; provided its amount is so small a fraction of unity that its diminution by eclipses, total or partial, may be neglected. In this formula, N is a number of globes of radius a uniformly distributed within a spherical surface of radius r. For the same quantity of matter in N′ globes of the same density, uniformly distributed through the same sphere of radius r, we have $\frac{N'}{N} = \left(\frac{a'}{a}\right)^3$; and therefore $\frac{\alpha'}{\alpha} = \frac{a}{a'}$. With $N = 10^9$, $r = 3{\cdot}09.10^{16}$ kms.; and a (the sun's radius) $= 7.10^5$ kms.; we had $\alpha = 3{\cdot}87.10^{-13}$. Hence $a' = 7$ kms. gives $\alpha' = 3{\cdot}87.10^{-8}$; and $a'' = 1$ cm. gives $\alpha'' = 1/36{\cdot}9$. Hence if the whole mass of our supposed universe were reduced to globules of density $1{\cdot}4$ (being the Sun's mean density), and of 2 cms. diameter, distributed uniformly through a sphere of $3{\cdot}09.10^{16}$ kms. radius, an eye at the centre of this sphere would lose only $1/36{\cdot}9$ of the light of a luminary outside it! The smallness of this loss is easily understood when we consider that there is only one globule of 2 cms.

diameter per 360,000,000 cubic kilometres of space in our supposed universe reduced to globules of 2 cms. diameter. Contrast this with the total eclipse of the Sun by a natural cloud of water spherules, or by the cloud of smoke from the funnel of a steamer.

Let now all the matter in our supposed universe be reduced to atoms (literally brought back to its probable earliest condition). Through a sphere of radius r let atoms be distributed uniformly in respect to gravitational quality. It is to be understood that the condition "uniformly" is fulfilled if equivoluminal globular or cubic portions, small in comparison with the whole sphere, but large enough to contain large numbers of the atoms, contain equal total masses, reckoned gravitationally, whether the atoms themselves are of equal or unequal masses, or of similar or dissimilar chemical qualities. As long as this condition is fulfilled, each atom experiences very approximately the same force as if the whole matter were infinitely fine-grained, that is to say, utterly homogeneous.

Let us therefore begin with a uniform sphere of matter of density ρ, gravitational reckoning, with no mutual forces except gravitation between its parts, given with every part at rest at the initial instant; and let it be required to find the subsequent motion. Imagining the whole divided into infinitely thin concentric spherical shells, we see that every one of them falls inwards, as if attracted by the whole mass within it collected at the centre. Hence our problem is reduced to the well-known student's exercise of finding the rectilinear motion of a particle attracted according to the inverse square of the distance from a fixed point. Let x_0 be the initial distance, $\frac{4\pi\rho}{3}x_0^3$ the attracting mass, v and x the velocity and distance from the centre at time t. The solution of the problem, for the time during which the particle is falling towards the centre, is

$$\tfrac{1}{2}v^2 = \frac{4\pi\rho}{3}x_0^3\left(\frac{1}{x} - \frac{1}{x_0}\right),$$

and

$$t = \sqrt{\frac{3}{8\pi\rho}}\left(\frac{\pi}{2} - \theta + \tfrac{1}{2}\sin 2\theta\right) = \frac{\pi}{2}\sqrt{\frac{3}{8\pi\rho}}\left[1 - \frac{2\theta}{\pi}\left(1 - \frac{\sin 2\theta}{2\theta}\right)\right],$$

where θ denotes the acute angle whose sine is $\sqrt{\frac{x}{x_0}}$. This shows that the time of falling through any proportion of the initial distance is the same whatever be the initial distance; and that the time (which we shall denote by T) of falling to the centre is $\frac{1}{2}\pi\sqrt{\frac{3}{8\pi\rho}}$. Hence in our problem of homogeneous gravitational matter given at rest within a spherical surface and left to fall inwards, the augmenting density remains homogeneous; and the time of shrinkage to any stated proportion of the initial radius is inversely as the square root of the density.

To apply this result to the supposed spherical universe of radius $3{\cdot}09.10^{16}$ kilometres, and mass equal to a thousand million times the mass of our Sun, we find the gravitational attraction on a body at its surface gives acceleration of $1{\cdot}37.10^{-13}$ kms. per second per second. This therefore is the value of $\frac{4\pi\rho}{3}x_c$, with one second as the unit of time and one kilometre as the unit of distance; and we find $T = 52{\cdot}8.10^{13}$ seconds $= 16{\cdot}8$ million years. Thus our formulas become

$$\tfrac{1}{2}v^2 = 1{\cdot}37.10^{-13}x_0\left(\frac{x_0}{x} - 1\right),$$

giving

$$v = 5{\cdot}23.10^{-7}\sqrt{x_c\left(\frac{x_0}{x} - 1\right)}$$

and

$$t = 52{\cdot}8.10^{13}\left[1 - \frac{2\theta}{\pi}\left(1 - \frac{\sin 2\theta}{2\theta}\right)\right];$$

whence, when $\sin\theta$ is very small,

$$t = 52{\cdot}8.10^{13}\left(1 - \frac{4\theta^3}{3\pi}\right).$$

Let now, for example, $x_0 = 3{\cdot}09.10^{16}$ kms., and $\frac{x_0}{x} = 10^7$; and therefore $\sin\theta = \theta = 3{\cdot}16.10^{-4}$; whence $v = 291{,}000$ kms. per second, and $t = T - 7{,}080$ seconds $= T - 2$ hours approximately.

By these results it is most interesting to know that our supposed sphere of perfectly compressible fluid, beginning at rest with density $1{\cdot}61.10^{-23}$ of that of water, and of any magnitude large or small, and left unclogged by ether to shrink under the influence of mutual gravitation of its parts, would take nearly seventeen million years to reach $\cdot0161$ of the density of water, and about two hours longer to shrink to infinite density at its centre. It is interesting also to know that if the initial radius is $3{\cdot}09.10^{16}$ kilometres the inward velocity of the surface is 291,000 kilometres per second at the instant when its radius is $3{\cdot}09.10^9$ and its density $\cdot0161$ of that of water. If now, instead of an ideal compressible fluid, we go back to atoms of ordinary matter of all kinds as the primitive occupants of our sphere of $3{\cdot}09.10^{16}$ kms. radius, all these conclusions, provided all the velocities are less than the velocity of light, would still hold: notwithstanding the ether occupying the space through which the atoms move. This would, I believe *, exercise no resistance whatever to uniform motion of an atom through it; but it would certainly add quasi inertia to the intrinsic Newtonian inertia of the atom itself moving through ideal space void of ether; which, according to the Newtonian law, would be exactly in proportion to the amount of its gravitational quality. The additional quasi inertia must be exceedingly small in comparison with the Newtonian inertia, as is demonstrated by the Newtonian proofs, including that founded on Kepler's laws for the groups of atoms constituting the planets, and movable bodies experimented on at the Earth's surface.

In one thousand seconds of time after the density ·0161 of the density of water is reached, the inward surface velocity would be 305,000 kilometres per second, or greater than the velocity of light; and the whole surface of our condensing globe of gas or vapour or crowd of atoms would begin to glow, shedding light inwards and outwards. All this is absolutely realistic except the assumption of uniform distribution through a sphere of the enormous radius of $3·09.10^{16}$ kilometres, which we adopted temporarily for illustrational purpose. The enormously great velocity (291,000 kms. per second) and rate of acceleration (13·7 kms. per second per second) of the boundary inwards, which we found at the instant of density ·0161 of that of water, are due to greatness of the primitive radius and the uniformity of density in the primitive distribution.

To come to reality according to the most probable judgment present knowledge allows us to form, suppose at many millions, or thousands of millions, or millions of millions of years ago, all the matter in the universe to have been atoms very nearly at rest† or quite at rest; more densely distributed in some places than in others, of infinitely small average density through the whole of infinite space. In regions where the density was then greater than in neighbouring regions, the density would become greater still; in places of less density, the density would become less; and large regions would quickly become void or nearly void of atoms. These large void regions would extend so as to completely surround regions of greater density. In some part or parts of each cluster of atoms thus isolated, condensation would go on by motions in all directions not generally convergent to points, and with no perceptible mutual influence between the atoms until the density becomes something like 10^{-6} of our ordinary atmospheric density, when mutual influence by collisions would begin to become

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* "On the Motion produced in an Infinite Elastic Solid by the Motion through the Space occupied by it of a Body acting on it only by Attraction or Repulsion." Cong. International de Physique, Paris, Vol. of Report. (*Phil. Mag.*, August 1900.)

† "On Mechanical Antecedents of Motion, Heat, and Light": Brit. Assoc. Rep. part ii. 1854; *Edin. New Phil. Jour.* i. 1855; *Comptes Rendus*, xl. 1855; Kelvin's 'Collected Math. and Phys. Papers,' vol. ii. art. lxix.

practically effective. Each collision would give rise to a train of waves in ether. These waves would carry away energy, spreading it out through the void ether of infinite space. The loss of energy thus taken away from the atoms would reduce large condensing clusters to the condition of gas in equilibrium * under the influence of its own gravity only, or rotating like our Sun or moving at moderate speeds as in spiral nebulas, &c. Gravitational condensation would at first produce rise of temperature, followed later by cooling and ultimately freezing, giving solid bodies, collisions between which will produce meteoric stones such as we see them. We cannot regard as probable that these lumps of broken-looking solid matter (something like the broken stones used on our macadamized roads) are primitive forms in which matter was created. Hence we are forced in this twentieth century to views regarding the atomic origin of all things closely resembling those presented by Democritus, Epicurus, and their majestic Roman poetic expositor, Lucretius.

ADDENDUM.—Lord Kelvin's paper to which he refers above, published in the *Philosophical Magazine* for August last, arrives at conclusions similar to those above. Tables of observed stellar velocities are given, which are seen to be consistent with the author's views. The following is extracted from the article :—

> Newcomb has given a most interesting speculation regarding the very great velocity of Groombridge 1830, which he concludes as follows :—" If, then, the star in question belongs to our stellar system, the masses or extent of that system must be many times greater than telescopic observation and astronomical research indicate. We may place the dilemma in a concise form, as follows :—
>
> " Either the bodies which compose our universe are vastly more massive and numerous than telescopic examination seems to indicate, or Groombridge 1830 is a runaway star, flying on a boundless course through infinite space with such momentum that the attraction of all the bodies of the universe can never stop it."
>
> In all these views the chance of passing another star at some small distance such as one or two or three times the Sun's radius has been overlooked ; and that this chance is not excessively rare seems proved by the multitude of Novas (collisions and their sequels) known in astronomical history. Suppose, for example, Groombridge 1830, moving at 370 kilometres per second, to chase a star of twenty times the sun's mass, moving nearly in the same direction with a velocity of 50 kilometres per second, and to overtake it and pass it as nearly as may be without collision. Its own direction would be nearly reversed and its velocity would be diminished by nearly 100 kilometres per second. By two or three such casualties the greater part of its kinetic energy might be given to much larger bodies previously moving with velocities of less than 100 kilometres per second. By supposing reversed, the motions of this ideal history, we see that Groombridge 1830 may have had a velocity of less than 100 kilometres per second at some remote past time, and may have had its present

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* Homer Lane, *American Journal of Science*, 1870, p. 57 ; Sir W. Thomson, *Phil. Mag.*, March 1887, p. 287.

great velocity produced by several cases of near approach to other bodies of much larger mass than its own, previously moving in directions nearly opposite to its own, and with velocities of less than 100 kilometres per second. Still it seems to me quite possible that Newcomb's brilliant suggestion may be true, and that 1830 Groombridge is a roving star which has entered our galaxy, and is destined to travel through it in the course of perhaps two or three million years, and to pass away into space never to return to us.

Real Paths of Meteors observed during the Perseid Epoch, July 13–*Aug.* 20, 1886–1901.

During the last 15 years a considerable number of meteors have been doubly observed during the progress of the Perseid display and their real paths computed. The results are of some little interest, not only as indicating the ordinary heights of these objects, but also as showing that minor showers are thickly distributed over the firmament. The following is a summary of 59 real paths computed by the writer; they include meteors from about 44 different systems, but these represent only a small proportion of the aggregate number of showers visible during the period embraced by the observations. Many of the more conspicuous of the minor showers are represented, and it is thought the radiant-points are pretty accurate, the observations having been specially made with the idea of securing duplicate records of the same objects. Every year furnishes additional material for the extension of such a list, and it is possible that its publication may be of use for reference and comparison during future returns of Perseids. It would appear that many, if not all, of the systems whose radiants are included in the summary, recur annually, and that an observer who maintains a close watch on the sky at the proper season will not fail to recognize a number of their members. Doubtless there are variations from year to year in the visible strength of the different showers, but something of them is to be seen every summer, and their apparitions may be expected with as much certainty as the return of the great display from Perseus. There is, of course, a great distinction as regards the comparative richness of the Perseids and its contemporary displays, for whereas the Perseids may furnish 100 meteors in an hour, some of the minor showers do not yield more than one meteor in five hours, or, in extreme cases, one meteor in ten hours. The observer must necessarily, therefore, watch the sky for long intervals in order to distinguish the more feeble streams. Short watches of the sky often give the impression that meteors are sporadic, and scattered broadcast over the firmament without any uniformity of divergence. But when observations are sustained over long intervals, and a sufficient number of meteors are recorded, the apparently discursive flights will be found to converge upon a

Real Paths of non-Perseids observed during the July–August Meteoric Epoch 1886–1901.

No.	Date.	G.M.T.		Mag.	Height at beginning.	Height at end.	Length of Path.	Velocity per second.	Radiant.		Observers.
		h	m		miles.	miles.	miles.	miles.	α. °	δ. °	
1 ...	1891 July 13	12	54	>1	82	50	154	......	58	+33	D. Booth, J. Sherwen.
2 ...	1899 13	10	55	3	75	69	15	21	283	−13	T. H. A., J. H. B.
3 ...	1900 15	10	13	2×♀	51	21	78	16	297	−11	W. F. D. and 3 others.
4 ...	1899 16	11	10	4	78	65	14	18	263	+36	A. S. H., W. F. D.
5 ...	1898 16	11	42	2–>1	79	37	46	21	273	+76	A. K., W. F. D.
6 ...	1900 17	8	47	☽	58	15	175	slow.	249	−20	17 observers.
7* ...	18	11	33½	♃	52	50	45	11	214	−10	A. S. H. and 2 others.
8 ..	1887 19	10	48	3–2	59	55	18	slow.	104	+52	J. E. W. F. D.
9 ...	21	11	24½	4	65	51	18	swift.	291	+47	J E., W. F. D.
10 ...	22	13	12½	1–♃	80	40	41	27	307	+44	D. Booth, W. F. D.
11 ...	1900 23	11	13	2–1	83	59	49	30	30	+47	A. K., W. F. D.
12 ...	23	13	1	2–1½	67	66	51	swift.	54	+9	W. E. B., W. F. D.
13 ...	24	10	49	3×♀	68	27	103	19	280	−15	W. F. D. and 4 others.
14 ...	24	10	42	3–2	72	63	28	12	305	−17	A. K., W. F. D.
15 ...	24	12	17½	4–1	78	71	39	49	14	−6	W. E. B., W. F. D.
16 ...	25	11	10	3–1	75	48	46	swift.	8	+39	A. S. H., W. F. D.
17 ...	26	9	10	½=☽	73	27	191	slow.	269	−23	F. C. Dennett, C. Grover, W. F. Kelsey.
18 ...	28	11	46	3–1	65	44	52	29	337	−10	A. S. H., W. F. D.
19 ...	28	12	35	4–3	84	76	32	40	16	−5	A. S. H., W. F. D.
20 ...	1899 29	11	9	4	55	54	12	13	317	−31	A. S. H., W. F. D.
21 ...	29	11	13	2–>1	62	47	17	27	323	+34	A. S. H., W. F. D.
22 ...	29	11	51	4	68	57	25	33	3	+10	A. S. H., W. F. D.
23* ...	1897 30	8	47	>♀	54	23	50	13	155	+59	7 observers.
24* ...	1898 30	10	53	4–3½	61	52	20	14	311	−10	A. S. H., W. F. D.
25 ...	30	11	10	3½–3	56	50	28	20	340	−18	A. S. H., W. F. D.
26 ...	30	11	35½	3–1	66	52	63	45	42	+23	A. S. H., W. F. D.
27 ...	1900 30	11	45	3–2	56	40	52	35	338	−10	A. S. H., W. F. D.
28 ...	1897 Aug. 1	12	14	2–1	55	42	25	medium.	296	−6	A. S. H., W. F. D.
29 ...	1899 1	11	1½	3–2	68	50	59	slow.	335	−11	J. H. B., W. F. D.
30 ...	1897 2	10	46	3–2	44	40	7	swift.	110	+70	H. C., W. F. D.

31 ...	1897 Aug.	2	10	52	4–2½	59	55	44	15	339	−24	A. S. H., H. C., W. F. D.
32 ...		2	11	24	5–4	139	124	28	swift.	73	+66	H. C., W. F. D.
33 ...		2	13	57½	3×♀	81	47	59	36	312	± 0	A. S. H., H. C.
34 ...		3	13	40	2–1½	60	54	61	37	72	+11	A. S. H., J. A. Hardcastle.
35 ...	1886	4	10	40	☽	90	11	168	21	161	+59	W. F. D. and others.
36 ...	1895	7	10	12	3–2	43	28	42	swift.	45	+47	H. C., W. F. D.
37 ...		7	11	4	1–♃	74	45	33	medium.	333	+36	A. S. H., H. C., W. F. D.
38 ...	1888	8	10	10	4–3	65	52	38	slow.	318	−13	G. T. Davis, W. F. D.
39 ...		8	11	16½	1	71	61	22	swift.	60	+48	T. W. Backhouse, D. Booth.
40 ...	1893	10	11	0	1	38	23	35	7	304	−13	H. C., W. F. D.
41 ...	1901	10	10	41	3–2	88	68	21	25	278	+67	A. S. H., W. F. D
42 ...		10	10	58	3–2	57	52	25	15	352	−14	A. S. H., J. H. B., W. F. D.
43 ...		10	12	19	3–2	72	60	33	25	149	+60	A. S. H., W. F. D.
44 ...		10	13	34	>1	63	29	48	16	269	+47	O. A. Le Bean, W. F. D.
45 ...	1898	11	8	58	♀	66	41	196	22	339	−10	H. C., W. F. D., and 12 others.
46 ...		11	11	52	2–1½	55	49	53	27	4	−20	A. K., W. F. D.
47 ...		11	11	57	3–2	56	39	25	36	57	+73	A. S. H., A. K.
48 ...	1893	14	11	0	1	60	43	15	slow.	294	+51	H. C., W. F. D.
49 ...		15	11	57½	♃	58	21	61	20	186	+74	H. C , W. F. D.
50 ...	1899	15	8	37½	2–1	73	48	27	swift.	334	+74	T. H. A., S. J. Johnson.
51 ...	1898	17	11	43	2–1	71	55	32	43	72	+52½	A. S. H., W. F. D.
52 ...	1901	17	10	12	3–2	72	66	27	45	58	+34	A. S. H., W. F. D.
53 ...		17	11	5	2½–2	58	53	23	32	95	+46	A. S. H., C. L. B.
54 ...		18	13	24½	4–3	62	50	28	15	106	+52	A. S. H., W. F. D.
55 ...		19	13	41	2	64	50	18	28	337	+ 3	A. S. H., W. F. D.
56 ...	1900	19	10	36	3×♀	56	29	62	...	346	± 0	T. E. R. Phillips, W. F. D.
57 ..	1901	20	11	8	1–>1	110	71	40	v. swift.	329	+61	P. M. Ryves, W. F. D.
58 ...		20	11	14	>♃	56	34	44	13	214	+49	C. J. Spencer, W. F. D.
59 ...		20	12	0	2	64	58	17	23	347	−15	A. S. H , W. F. D.

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* Exact position of the radiant-point somewhat doubtful.

Nos. 3, 14, 40 are α Capricornids. Nos. 18, 25, 27, 29, and 45 are σ Aquarids.

Abbreviations:—

A. S. H.	Prof. A. S. Herschel, Slough.	J. H. B.	J. H. Bridger, Farnborough.	J. E.	J. Evershed, Kenley, Surrey.
H. C.	H. Corder, Bridgwater.	A. K.	A. King, Leicester.	W. F. D.	W. F. Denning, Bristol.
T. H. A.	T. H. Astbury, Wallingford.	W. E. B.	W. E. Besley, London, S.W.		

series of well-defined centres of emanation, each of which represents the focus of a meteor-shower.

The real paths of Perseids have been purposely excluded from the table, as a great number of these have been computed, and our present object is to deal with the secondary showers. It may be briefly stated, however, that unmistakable Perseids have been doubly observed between July 19 and August 15, and that the radiants determined from them agree very satisfactorily with the shower-radiants in proving the easterly motion of the centre.

Bishopston, Bristol, W. F. DENNING.
1901, Sept. 4.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Eclipse of Thales.

GENTLEMEN,—

I had not intended to write any more upon this subject, believing that all that could be said upon it had been said. But Prof. Stockwell, in the last number of *Popular Astronomy*, has revived the claims of another candidate for this phenomenon, and perhaps a few words will be expected from me.

I shall not begin by disputing the name of the eclipse in question. It will probably be called that of Thales to the end of time, though I may be excused for rejecting the statement that he predicted its occurrence. No one ever said that he did more than indicate the *year* in which it was to take place; and to me it seems almost absurd to suppose that if, by any cycle or otherwise, he really did foresee it, he should not have announced at least the month of its appearance. But let that pass for the present. It is well known that, according to Herodotus, during the war between the Medes and Lydians, a sudden darkness occurred when a battle was joined, which caused the combatants to cease fighting and disposed their leaders to seek peace. That such a darkness would take place in that year had been predicted, the historian asserts, by Thales; and the idea, which seemed probable, that it was caused by a total, or nearly total, eclipse of the Sun has led to many attempts to ascertain when a large solar eclipse occurred in that locality and thereby fix the date of the battle.

Prof. Ginzel, in his 'Spezieller Kanon der Sonnen und Mond Finsternisse' (p. 169), gives a list of the eclipses suggested by the older chronologists. Petavius was the first of these, and he contended for that of B.C. 597 July 9; Riccioli afterwards expressed a decided preference for that of 585 May 28; Scaliger supported that of 583 October 1, whilst Costard, Stukeley, and Mayer fixed upon that of 603 May 18. But the first to discuss the matter with the aid of modern tables of the motions of the Sun and Moon was Baily, who read a paper (the first of his written contributions to science) on the subject to the Royal Society on the 14th of March, 1811, which is printed in *Phil. Trans.* vol. ci. p. [220]. He is very emphatic on the necessity of supposing that the eclipse was total, and considers that he has shown that this condition is fulfilled only by the eclipse of B.C. 610 Sept. 30. Nevertheless he is somewhat puzzled by the fact that he cannot find that the eclipse of B.C. 310 Aug. 15 was total in any part of the course followed by Agathocles in his voyage from Syracuse to Africa; and therefore thinks that some alteration is required in the table of the secular variation of the Moon's mean distance from her node. The tables used by Baily were the *Tables Astronomiques* of Bürg, then recently published in France, and afterwards inserted (adapted to Greenwich) in Vince's 'System of Astronomy.'

A consideration of the above circumstances led Bosanquet to point out in the *Athenæum* (for 1852 Aug. 7) that there was no sufficient reason of rejecting the date B.C. 585, corresponding to that given by Pliny for the date of the battle, and that the eclipse was in fact that of May 28 in that year. Hind upon this calculated the circumstances of that eclipse, from Burckhardt's lunar tables, with certain corrections resulting from Airy's discussion of the Greenwich observations and from Hansen's researches, his conclusions being that the eclipse of B.C. 585 satisfied all the conditions required by history. Airy himself published a very interesting paper on the subject in the *Philosophical Transactions* for 1853 (which was in preparation when Hind's paper was printed). He says that he attached great importance in the rectification of the lunar elements to the historical account of the eclipse of Agathocles; and that having done so, he fully accepted the date B.C. 585 May 28 as that of the eclipse of Thales.

It has been suggested that as the period of eclipses sometimes called a saros (18·03 years, or 223 lunations) intervened between B.C. 603 May 18 and B.C. 585 May 28, Thales may have been acquainted with this period and predicted the latter as probable from a knowledge of the former. But I cannot attach any weight to this idea, because the period in question gives no indication as to what part of the world (of which a small portion only was known in those times) the future eclipse would be visible. I was struck by a remark of Hind in the letter above referred to in the *Athenæum*, viz. that "it is questionable whether the effect would not be more imposing near sunset than at an earlier time of the day." For the eclipse of 585 occurred in the evening; and if not quite total at the place of combat it would certainly attract more attention if the Sun were approaching the horizon.

We will now consider Prof. Stockwell's paper. He has for some time past convinced himself that the values of the secular acceleration of the Moon's motion and of the motion of the perigee used in our present lunar tables are erroneous, and has constructed new eclipse-formulæ founded on his own values of these elements. These appear to show that the eclipses of B.C. 610 Sept. 30 and B.C. 603 May 18 were both total in Asia Minor, the latter for a longer duration than the former. Either, then, of these he thinks would do for the eclipse which, according to Herodotus, occurred at the time of the battle. But he suggests a criterion for deciding between them, founded on the supposed prediction of Thales. He has noted that cycles of eclipses are completed at the end of successive periods of 82 lunations at the same node and 88 lunations at opposite nodes. Now one of the former of these periods elapsed between B.C. 610 Sept. 30 and 603 May 18, and the suggestion is that Thales observed the eclipse at the former date, and being acquainted with the cycle in question predicted the eclipse at the latter date, which is therefore the eclipse of Thales. Now it is a very remarkable coincidence that this cycle should give two consecutive eclipses which were total at or near the same locality. Amongst the remarkable eclipses visible in the last sixty years (the appearance of which in any part of the world is noticed) there are in all four instances of two occurring at the beginning and end of this cycle, the first being those of 1851 July 28 and 1858 March 15; the central line of the former passed across Scandinavia and the Baltic; that of the latter over England. The second case is that of the eclipse of 1871 Dec. 12 and 1878 July 29; the former of which was total in Southern India and Australia, the latter in North America. The third instance is furnished by the eclipses of 1883 May 6 and 1889 Dec. 22; the central line of the former was confined to the Pacific Ocean (it was observed only on Caroline Island) and that of the latter passed across the Atlantic from South America to the west coast of Africa. The fourth instance of two great eclipses occurring of late years at the beginning and end of this cycle is that of the pair of 1894 Sept. 29, which was total only in the Indian Ocean, and the recent one of May 18 in the present year, which was very much larger and had a central line considerably to the south of the former. It seems very unlikely, then, that Thales should have had a sufficient record of eclipses to lead him to expect one following that of B.C. 610 Sept. 30 at the end of this cycle. As to the cycle of 22,325 lunations, or 1805 years, detected by M. Oppert and designated by Mr. Crommelin the megalosaros, we may safely say that the Greek philosopher could not have known of it.

With regard to Prof. Stockwell's Note I., I may be allowed to refer to my letter in the *Observatory*, vol. vii. p. 291, at the end of which I pointed out that Cicero stated that the eclipse occurred in the reign of Astyagês of Media, and Pliny gave a date corresponding to B.C. 585 in our chronology, both statements being consistent with each other. As the Medo-Lydian war was probably subsequent to that in which Nineveh was taken by the

Medes and Babylonians, that year is more likely, on historical grounds to be the date of the battle than B.C. 603.

Yours faithfully,
W. T. LYNN.

P.S.—It may be worth while to mention that Baily was not, as Prof. Stockwell calls him, *Sir* Francis.

Blackheath, 1901, Oct. 7.

Anomalous Occultations.

GENTLEMEN,—

In the current volume of the *Observatory* are to be found a number of allusions to anomalous occultations of stars by the Moon, as on pp. 94, 186, 210, 313–316, 345–346, and to the possibly allied phenomenon of Baily's Beads in vol. xxiii. pp. 430–431 and vol. xxiv. p. 62.

If the writers of these various articles will refer to a paper * of which I give below a brief review, I am sure that they will find much that will interest them.

The author narrates many personal experiences during fifty-four years as an astronomical and geodetic observer in many parts of the United States, and makes many references to observations by others—all treating of the phenomena referred to in the title.

The following topics are particularly referred to :—

(1) The projection of red stars on the bright limb of the Moon, so that the disappearance is often $2^s{\cdot}5$ later than the time of apparent contact with the bright limb.
(2) A similar projection, sometimes for several minutes, of stars or satellites on Jupiter.
(3) A similar projection of the Moon on the Sun or corona at a total eclipse.
(4) The "atmosphere-ring," "the black-drop," &c. of Venus or Mercury at a transit across the Sun's disk.
(5) On the empirical correction to the semi-diameter of the Sun, or Moon or planets, in the Ephemerides.

These phenomena are explained by the author, with much show of probability, as resulting from the *unsteadiness of the atmosphere on the Earth, near the observer,* instead of being due to any of the many causes assigned by other astronomers. Many illustrations of similar phenomena in geodetic operations, where the cause could be unmistakably assigned, are adduced to lend weight to the author's very plausible theory. Hence he arrives at the conclusion that for precision in such observations an exceedingly serene atmosphere (best found at great elevations) and selected moments of steady seeing are requisite: that observations under other conditions should be discarded as introducing a systematic error: that in all cases the measured diameter of the Moon, Sun, or planet will be too large; but that this actual increment of the apparent image will be greater, the greater is the atmospheric unsteadiness; and that the variations in the amount of this increment due to differences of the steadiness are much greater than the variations of the probable errors, which represent the uncertainty of a pointing of the micrometer wire by reason of the lack of sharpness of the limb when the observations are made.

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* George Davidson, "The Apparent Projection of Stars upon the Bright Limb of the Moon at Occultation, and similar Phenomena at Total Solar Eclipses, Transits of Venus and Mercury, &c., &c.," Proceedings of the California Academy of Sciences, 3rd series, vol. i. pp. 63–102, 8vo (issued December 1, 1900).

International Latitude Observatory,
Gaithersburg, Maryland, U.S.A.,
1901, Sept. 28.

HERMAN S. DAVIS.

Daylight Meteors.

GENTLEMEN,—

Like Mr. Coles, I have observed daylight meteors (?), and possibly my experience may enable him to account for the phenomenon witnessed by Mrs. Coles and himself on September 18.

Some years ago, when scrutinizing the Sun's limb—which at that particular instant I had suffered to travel just out of the field—I was startled to see a shower of luminous objects driving rapidly and nearly horizontally, which must, from the direction of their drift, have apparently crossed the Sun's disc immediately afterwards. They were notably bright; but as I was struck with the imperfection of the definition of them, and with the fuzziness of their tiny discs, it occurred to me to try to focus accurately upon them. On doing so, I found at once that the eyepiece had to be racked out, and on obtaining the true focus for my meteors (?) discovered that I was looking at hundreds of pieces of *thistle-down* driving on the wind, and brilliantly illuminated by the sunlight. Having once seen them clearly and sharply defined and satisfied myself as to their nature, I discontinued my watch, so am unable to say how long the shower lasted. Inasmuch as the thistle-seeds were probably less than 30 or 40 yards from the object-glass of my equatorial, and the Sun was more than 92,000,000 miles off, the necessity for a considerable change of focus will be at once apparent.

Should Mr. Coles witness a recurrence of his daylight shower, perhaps I may venture to suggest that he should repeat my experiment of focussing, not on Arcturus, but on the objects themselves.

Forest Lodge, Maresfield,
Uckfield, 1901, Oct. 4.

Yours faithfully,
WILLIAM NOBLE.

[Since the above has been in type we have seen a correspondence on the same subject in a contemporary, in which Mr. Coles gives reasons for thinking that the objects could not have been terrestrial, and gives reference to a similar observation many years ago by Mr. Grover, when the observer was equally confident that he was not deceived; but several persons are of the same opinion as Capt. Noble.—Eds.]

The Efficiency of the Curved Plate for Charting Purposes.

Gentlemen,—

Prof. Turner's reply to my note on the above subject in the July number of the *Observatory* evidences, to say the least, a hasty perusal. He says "the number of plates required to cover the sky is *not* to be obtained by dividing the area of the sky by the area of the plate. You must fit the plates in so as to 'cover the sky,' and this means a certain amount of overlap." Prof. Turner overlooks entirely the fact that I expressly stated in my note that the figures I gave were for the number of plates required to "cover" the surface of a sphere and of a hemisphere, "*allowing nothing for overlapping.*" The figures I gave represent accurately the comparative numbers of plates required to "cover" the given areas under the conditions specified. If other conditions are assumed, then these figures must be either *decreased* or *increased*, according to the form of the plate and the amount of overlapping provided for. But as the decrease or increase is proportionally the same (nearly) for all three types of instruments, I did not deem it necessary in my previous note to give several sets of figures, all having practically the same ratio, for the purpose of illustrating such a simple point as that in discussion. It seems, however, desirable to do so now in view of Prof. Turner's remarks.

1st. If square plates are used (the side of the square being taken as the diameter of the circle of good definition), the number of plates required to "cover" (in theory and practice both) a given region of the sky will be less than the number given in my preceding note in the ratio of $\pi : 4$, *i. e.* the figures for a hemisphere and sphere will become respectively:

	10·3,	827,	and	5240,
and	20·6,	1654,	and	10480.

Prof. Turner says "I took square plates because they were more favourable to Prof. Wadsworth's figures." This is a curious statement in view of the above result and his original criticism. What he really did apparently was to compare the figures obtained for the 45° *circular* curved plates with the figures for 2° *square* plates, of which he states 11000 would be required, accurately 10480 as shown above. I pointed out this error in my previous note, in which I also stated the objection to the use of square fields as a basis of comparison. Square plates will, it is true, "cover the sky" in practice with the least amount of overlapping, but if the side of the plate is equal to the diameter of the circle of good definition (so as to include the whole of this area on the plate) the definition in the corners is bad, and in order to cover the whole region satisfactorily we need to take a second set of plates whose centres are at the corners of the first set, as in the work of the Astrophotographic Survey. On the other hand, if we use smaller square plates (so as to cut out the bad corners), we do not get the full good field of the lens at the sides. As stated before, therefore, "in comparing different lenses, the diameter of the maximum circular field of good definition" (and results based thereon) "is obviously the only proper criterion."

2nd. If circular plates are used, and *allowance is made for the necessary overlapping* required to "cover the sky in practice," the numbers given will be increased in a ratio depending on the relative arrangement of the centres of the plates. If the centres are so placed that the edges of the circular fields of adjacent plates are just tangent at the corners of the inscribed square, the numbers will be increased in the ratio $\pi : 2$, *i. e.* they will become respectively:

For the hemisphere: 20·6, 1654, and 10472.
For the sphere: 41·2, 3308, and 20944.

These figures are 57 per cent. greater than the ones given, with (as stated) "*no allowance for overlapping.*" In this connection, I think I ought to "*plead*" with Prof. Turner "*for a little more accuracy in numerical statement.*" He says: (with) "*circular plates he must increase*" (to allow, presumably, for the overlapping mentioned above) "*his figures to nearly double, perhaps more than double.*"

The figures given above are not quite as favourable to the large field-plates as they would actually be "*in practice,*" owing to the fact that we are dealing with overlapping on the surface of a sphere and not a plane. The necessary allowance for overlapping will grow less and less as the size of the field increases, until at 180° it becomes *nil.* For a field of even 45° the gain would be sensible, roughly about 10 per cent., so that the relative number of plates required to cover say a hemisphere for fields of 45° and 2° diameter would be in the ratio of about 1 to 550, instead of 16 to 7000 or 1 to 437, as given in my report. Therefore instead of over-estimating the advantages of the curved plate as compared with others, as Prof. Turner attempted to show in his criticism of my figures, I really considerably underestimated them. To this extent, therefore, I am certainly obliged to him for his attempts to prove me wrong.

As regards Prof. Turner's last statement in regard to "my attempt to defend them" (certain dates) "in a very strained manner," I only wish to remark that here, again, Prof. Turner has entirely missed the point of what he is pleased to term my "defence." Prof. Turner in his first criticism implied that the language of my report reflected unfavourably on the progress, &c. of the work of the International Survey. It was this implication which I objected to and answered, and not "his friendly correction (?) of one or two figures and dates." I do not think there was anything expressed or implied in the general statements made in my report which called for his original criticism and the discussion which has resulted therefrom. I hope, at least, that I have now made the meaning of all these statements so clear that the discussion will not need to go farther. In the future I certainly will not take Prof. Turner "seriously" unless he labels his notes to that effect. "Not to be taken seriously" or *vice versâ*, appearing over each of the items "From an Oxford Note-Book," would remove a considerable amount of uncertainty as to just what Prof. Turner does frequently mean, and add that much enjoyment to the perusal of that always interesting part of the *Observatory*. Yours faithfully,

Allegheny Observatory,
Sept. 1901.

F. L. O. Wadsworth.

[I am quite ready to fall in with Prof. Wadsworth's suggestion that this discussion need go no farther.—H. H. T.]

OBSERVATORIES.

Royal Observatory, Cape of Good Hope.—Sir David Gill has got two new instruments, both of great importance, on hand, about which he reports progress in his Report to the Admiralty, dated 1901 January 31, for the year 1900. The new transit-circle had not arrived at that date, but was probably on its way; the house to contain it, designed by Sir David, and constructed by Messrs. Cooke & Son, was just completed. That this is a novelty in observing-rooms will be judged from the description:—

> It is of sheet-steel, having triple sides, which form a double series of ventilating-shafts, and these shafts are so designed as to carry off all heated air by convection and to deliver it by ventilating-funnels 13 feet distant from the observing aperture. The intention of this arrangment was to insure equality between the external and internal temperatures, a result which has been fully attained. The observing aperture 6 feet wide is formed by rolling the two halves of the building apart, so that the instrument may be used practically in the open air. An internal wind-screen 7 feet high is provided for the protection of the observers, but there are doors in the meridian, which leave a free aperture when desired.

The second of the new instruments, the McClean equatorial and spectroscope, is much further advanced, but still has not yet wholly passed the experimental stage. The refiguring of the lenses has been done by Sir Howard Grubb, and the object-glass had begun its return journey to the Cape. It had been found from observations with the "line of sight" spectroscope in 1899 that, in consequence of the heavy-flint-glass employed for the construction of its three prisms, the absorption of light was so considerable as to render undesirably long exposures necessary. Mr. McClean has generously provided for the construction of a battery of four prisms of lighter and more transparent glass to replace these. Mr. McClean has also arranged with Messrs. Zeiss for the construction of an object-glass prism with refracting angle of 10°, to be used in conjunction with the already existing prism of 8½° angle, in order to distinguish the spectra of helium stars from those of other stars of Type I.

Whilst the photographic telescope was thus deprived of its objective, the 18-inch visual telescope was fitted with the Repsold micrometer and used for the observation of double stars. Mr. Innes detected 31 previously unrecorded pairs, of which 9 were naked-eye stars. The same instrument was also used to search for Comets Brorsen and Finlay. In looking for the first of these, five previously unrecorded nebulæ were found.

The transit-circle was used during the year 1900 chiefly in making a thorough determination of the observers' personal equations in right ascension as affected by the star's magnitude. The result of these is to show generally that the observers record faint stars relatively later than bright ones (three observers, however, appear to make no distinction between stars of magnitudes 0 to 4), and that the change of personality is as a rule smaller per magnitude for the brighter than the fainter stars. The observations with the heliometer of the major planets at Opposition have been continued, and the Astrographic Catalogue work is proceeding. 124 plates were measured in each of two positions of the plate during the year. This appears to be the output of two Repsold measuring-machines, the second of which was brought into use early in the year, but additions to and changes in the *personnel* of this department did not permit full work being done with these.

Sir David Gill devotes several paragraphs to a description of the state of the work on the Geodetic Survey of South Africa. The Survey of Rhodesia and the Anglo-German Boundary Survey are both being pushed on. One most interesting statement refers to the completion of the great African arc of meridian, or, in other words, the survey from Cape to Cairo. Mr. Rhodes has promised to supply funds for the completion of a geodetic arc along the 30th meridian from the Limpopo to the southern end of Lake Tanganyika; and Sir David Gill says: "The execution of the work from the north of Natal to the Limpopo is now rendered almost certain by the fact that it has the fullest support of Sir Alfred Milner, and will probably be begun so soon as the Transvaal is in a sufficiently settled state."

MELBOURNE.—Mr. Baracchi's report on this Observatory for the period 1900 April 1 to 1901 March 31 shows that the Astrographic Catalogue forms the staple new work being now done at Melbourne. Five young ladies are occupied in measuring the plates taken at the Sydney and Melbourne Observatories. (It is noted in Sir David Gill's report that the region forming the polar cap from the pole to 2° S.P.D., originally allotted to Melbourne, has been handed over to the Cape Observatory.) They use a Repsold measuring-machine at Melbourne, and, having got into good practice, they can measure 170 stars in an hour; so that in a day of six or seven hours a plate containing 500 stars can be completely measured in two positions of the instrument, by two persons, each acting as recorder for the other in turns. At the date of the Report, 146 plates had been measured containing about 74,000 stars. The transit-circle is also devoted to the same work, for with it the places of the fundamental stars for reduction of the plates are being determined, and about 1100 such observations, excluding the necessary observations of azimuth and clock-stars, were made during the year.

This, save for some occasional observations with the great equatorial, is the total of the astronomical work. The meteorological work and the Time Service were continued as before, and the reduction of the observations of terrestrial magnetism made since 1868 is proceeding. There are also schemes on foot for the reduction of tidal observations and for seismological observations.

The post of Chief Assistant has not yet been filled up, but it has been urged on the Government that it is time that this was done. A committee of the Board of Visitors formulated the qualifications necessary for the occupant of the post, and these, as given in the Report, seem to us not to err on the side of modesty. The man the committee ask for is to be young (not more than 35 years of age), sound in health, with perfect eyesight, a thorough mathematician, who is able to apply his knowledge to theoretical and practical astronomy, and is to be experienced in the making of astronomical observations. Might we humbly suggest to the Committee that they may have some difficulty in finding such a paragon who would be tempted by a salary on the scale which has hitherto obtained in Colonial observatories!

SOUTHPORT. — On Wednesday, September 4, the Mayor of Southport opened an astronomical observatory in Hesketh Park, the gift of Mr. Joseph Baxendell, Borough Meteorologist, together with the Fernley Trustees, the latter giving £100 for erection of the substructure. The observatory consists of a lower storey divided into two rooms, one forming a long-needed computing-room for the Meteorological Department, and the other a store-room for both observatories. Upon these rooms is mounted as an upper storey an ornamental wooden astronomical observatory, given by Mr. J. Baxendell, consisting of an equatorial room with revolving dome combined with a transit-room. The principal instruments are an equatorially-mounted achromatic refracting telescope of 6 inches aperture, by Messrs. Cooke & Sons, of York, and a transit-instrument by Messrs. Troughton & Simms. The assembled company, among whom were the Rev. W. Sidgreaves, of Stonyhurst, and Mr. E. Crossley, of Halifax, heartily seconded the vote of thanks to Mr. Baxendell, proposed by the Mayor, Dr. Isherwood.

PUBLICATIONS.

A Text-book of Astronomy *.—A new century has much to answer for. In particular the Twentieth Century is made responsible for the issue of a new series of text-books bearing its name, " in order to meet the new demands of the country along the higher plane of educational work." Further, the publishers aver that the offer of these text-books " is an event worthy of the twentieth century." As far be it from us to speak disrespectfully of the twentieth century as of the equator, but in all humility we would urge that the Twentieth Century is rather young for us to say of what she is or is not worthy. Her mother, the Nineteenth, departed this life in rather bad odour, with famine, pestilence, and war to attend her obsequies, and, so far, the daughter has walked carefully in the footsteps of her mother.

This is, however, apart from the value of the text-book of astronomy that is the particular number of the series now before us. Mr. Comstock has given us a most excellent treatise; it is comprehensive, complete, and accurate. In a word it covers the whole range of the " new astronomy," and goes into each detail of its different branches. It is well and generously illustrated; its paper and print are good; and, including preface, subject matter, appendix, and indexes, it occupies four hundred pages. It does not deal with the old or mathematical side of astronomy to any appreciable extent, but a student at school or college who is reading up for an examination on the physical or descriptive side of astronomy will find in it all the necessaries for his purpose. But this does not make the book a readable one, just as meat tabloids are not very palatable—nor, indeed, very digestible; in both cases the nutritious matter is too condensed.

The discussion of theories does not of course find much place here. In one instance, however, Mr. Comstock has allowed himself to be led aside on the question of life in other worlds. It seems to us that he is himself guilty of a mere play upon words when he says that " it is an abuse of language to say that life in Mars may be utterly unlike life on the Earth; if it is absolutely unlike it is not life, whatever else it may be." That simply depends on Mr. Comstock's definition of " life," which is by no means a necessary and sufficient one.

It is interesting to find that Mr. Comstock regards the incident at Beth-horon, when " the Lord cast great stones from heaven upon " the Amorites, and they " died with hailstones," as an early record of the passage of the Earth through the nucleus of a comet. We are astonished to see that Mr. Comstock alludes with apparent seriousness to the *nine* satellites of Saturn. As regards the ninth satellite, we thought that all astronomers held with Mrs. Betsy Prig on the subject of this astronomical " Mrs. Harris."

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* By George C. Comstock, Director of the Washburn Observatory and Professor of Astronomy in the University of Wisconsin. London: Hirschfield Brothers, 13 Furnival Street. 1901.

NOTES.

Comet Notes.—Prof. Holetschek, of Vienna, gives the following measures of the brightness of Encke's Comet (*Ast. Nach.* 3745):—

Date.	Whole light. mag.	Light of nucleus. mag.	Time when comet disappeared in dawn. h m	
1901 Aug. 18	8·1	9½	15 41	Local M.T.
19	7·7	...	15 43	„
22	7·6	8½	15 57	„
23	7·8	9	16 0	„
25	8	...	16 0	„
Sept. 2	6½±	8	16 15	„

After perihelion passage the comet ran south, and could only be observed in the southern hemisphere. A. C. D. C.

Minor Planet Notes. — The following planets have been discovered:—

Designation.	Discoverer.	Place.	Date.	Mag.
GV	Villiger.	Munich.	Oct. 11	11·5
GX	Carnera and Kopff.	Heidelberg.	„ 11	13
GY	„ „	„	„ 11	12·8
GZ	Kopff.	„	„ 13	12·5
HA	Carnera and Kopff.	„	„ 13	13
HB	Kopff.	„	„ 16	13·7

A planet that was designated GW proved to be identical with (247) Eukrate.

'Harvard Circular' No. 61 contains particulars of the next apparition of Eros, from which we take the following extracts:—

	R.A. h m	Dec. °	Mag.	
1902 Sept. 27	8 42	N. 25·0	12·45	
Nov. 16	11 37	N. 2·3	12·09	Perihelion.
1903 Jan. 5	14 9	S. 21·7	12·07	
Feb. 24	16 30	36·1	12·11	
Apr. 19	18 5	43·1	11·79	
June 4	17 14	43·5	11·55	Nearest Earth.
July 24	16 21	31·1	12·41	
Sept. 12	17 17	S. 25·4	13·43	

Thus the planet may be observed in this country in the autumn of 1902, but at the time of its nearest approach it must be left to southern observers. It is most important to observe the planet at every opposition, both in order to determine the perturbations by the Earth, which in the course of years should give an excellent determination of the Sun's distance, and to unravel the mysteries of its variation of light. A. C. D. C.

Obituary.—Alfred W. Drayson. We have no hesitation in including the name of Lieut.-Col. Drayson, whose death has recently occurred, in these pages, for though his views on some points were unorthodox, he was without doubt well read in astronomy, and must hence be considered an astronomer. His works are usually classed under the head of paradoxical literature, but he was not by any means in the same category with those who think that the Earth is flat, and such folk.

Capt. Drayson, of the Royal Artillery, was at one time an instructor in Military Surveying and Topography at the Royal Military Academy, Woolwich, and being therefore acquainted with practical geodetical work, the fact that the measurements of certain distances at different times gave different results seems to have suggested to him that the Earth was growing rapidly. He published a book, entitled "The Earth we Inhabit," in the year 1859, in which he produced arguments in favour of this idea. One remarkable point was attempted to be made from the breakages of the submarine cables, which the author thought was a natural result of the swelling of the globe. In the same book Drayson endeavoured to show that the size of the Earth's orbit was increasing and hence the year lengthening, which he thought would account for the longevity of the patriarchs as given in the biblical narrative; but at this period he seems to have had no objection to the accepted doctrine of "the precession of the equinoxes," which was a subject on which he wrote a book, 'The Motions of the Fixed Stars,' a dozen years later. In this he set out an hypothesis that the pole of the Earth is not describing a circle about a moving pole of the ecliptic, but that it is describing a circle about a fixed point about 29½° from the pole of daily rotation. This hypothesis, although it may satisfy the observed difference in the obliquity of the ecliptic, for a comparatively short period of time, has no dynamical interpretation, and it must be remembered that the chief proof of the correctness of the orthodox precessional geometry is its accordance with the laws of gravitation. Drayson pointed out that if his theory were the correct one it would account for the happening of glacial periods as demanded by geology; and this has been sometimes considered a point in its favour. It will be judged from this that Col. Drayson was a man of some intellectual power. He was the author of other books of quite a different nature—'Sporting Scenes in South Africa' is described by its title, and a treatise on Whist, of which game he was an excellent player, showed much smartness and originality. He was elected a Fellow of the Royal Astronomical Society in January 1868, and contributed a paper on "A Uniform Measure of Time."

Nova Persei.—The new star in Perseus is now being observed by many persons, and a good quantity of literature is appearing relating to it. 'Bulletin No. 8 of the Lick Observatory' gives its magnitude from February 24 to September 13, and a detailed account of its spectrum up to August 11. At the present time it may be said that it is varying between the 6th and 7th magnitude, as it has been for three months past. Father Sidgreaves gives a summary of recent photographs of its spectrum as follows :—

All hydrogen lines are now relatively weak, excepting the doubtful line Hϵ.

λ5007	much stronger than Hβ or Hγ. Great width.
4958	prominent broad band.
4718	grown from a weak line into a strong band, with a strong line (4713) on its edge.
4688	rather weak broad band.
4640	gradually weakened like hydrogen.
4364	very prominent band, stronger than Hγ, crossed by three bright lines.
3969	Hϵ? As strong as all other hydrogen lines together.
3869	stronger than 3969.

All these, with the exception of 4718, which shades off on red side, are broad with sharp edges. The structure of bands 3969 and 3869 very remarkable, being crossed by four strong lines of the same relative intensities and at the same intervals. This is also shown in the line 4364.

STEEPNESS OF PHOTOGRAPHIC PLATES.—A valuable paper by Sir William Abney, in the *Phil. Trans.* for March 2 last, may be found useful in investigations of star-magnitudes by photography. It has sometimes been assumed-in such that equal diameters result if, when the intensity of the light is diminished, the time of exposure is increased in the same ratio. The "gradation of a plate" may be taken as the diminution of transparency of the deposit, or increase of density of the image as the time of exposure is increased. That is, if small squares on a plate be exposed for 2 sec., 4 sec., 8 sec., &c. respectively, and their transparencies be measured and set up as ordinates with abscissæ 2, 4, 8, &c., the resulting curve is the gradation of the plate. Now Sir W. Abney says, quoting from an earlier paper, that supposing a light of unit intensity to give a certain density on a plate exposed to it for one second, if the intensity be reduced to $\frac{1}{100}$th of its former value, and the exposure be increased to 100 seconds, the density will not be the same in both cases, although the plate gradation is the same for both intensities. The result of the present work seems to show that the gradation of the plate depends on *the kind of light* to which it is exposed. With one kind of plates experimented with, the maximum sensitiveness was found to be near H in the solar spectrum, and it was to the light of these rays that the gradation was found to be least. In the blue, where the sensitiveness of the plate was small, the gradation was excessively steep.

STEALING THE STARS.—The following paragraph under this heading appeared in a daily paper :—

"A St. Petersburg despatch received in Paris states that the arrival of a band of Russian astronomers at Tomsk, in Siberia, where they are about to establish an observatory, has caused an outburst of hostility among the peasants in the neighbourhood. The natives are convinced that the astronomers will gather the stars together in a bag and take them away, thus causing droughts, since the rain, they believe, comes from the stars."

THE DIAMETERS OF VENUS AND MERCURY.—Mr. Drew with the Lowell 24-inch refractor finds the diameter of Venus to be 16″·90. A peculiar feature of the 139 measures is that the arrangement in order of time gives 16″·63, 16″·75, 16″·91, 16″·82, 17″·08, 17″·03, 16″·97, which Mr. Drew considers more than a mere coincidence.

Dr. See has now published, in *Ast. Nach.* 3737, his preliminary researches on the diameter of Mercury. The measures from 1630–1894 are collected, and those made by him with the 26-inch Washington refractor added. The diameter at mean distance from the Earth which results is practically 5″·9.

MESSRS. PHILIP & SON, of Fleet Street, have sent us specimens of globes which they are now selling at three shillings per pair. The spheres, mounted with freedom of rotation, on slight brass stands, are of about 4½ inches diameter. As to the celestial globe, the constellations are so arranged that, when it is rotated in the direction of diurnal motion, the stars of smaller R.A. precede, as in nature; whereas, in some cases, a celestial globe is a succession of planispheres, and, so far as relative position is concerned, the picture on the *outside* of the sphere is exactly like the sky, Taurus, when on the meridian, having Aries on the right and Gemini on the left. In the globe now in hand the reverse is the case. Each plan has its advantages. As to the terrestrial globe, it need only be said that, though small, it is easily legible.

IF anyone wants to know when a total solar eclipse will next be visible in England, or any remarkable fact connected with any past eclipse, he should invest sixpence in the latest edition (the sixth) of Mr. Lynn's 'Remarkable Eclipses,' just published by Messrs. Sampson Low. As the author says, the book consists of a historical and chronological sketch of eclipses before their scientific study began, which may be said to date from 1715, and a rapid survey of those which have occurred since, including that of May of the current year.

THE following is from a Sussex newspaper of October 19:—"The marriage of Dr. Isaac Roberts, F.R.S., of Starfield, Crowborough, and Miss Dorothea Klumpke, of San Francisco, late of the Bureau of Measurements at the National Observatory, Paris, took place at Uckfield, on Thursday. The bride was accompanied by her mother and her sister, Miss A. E. Klumpke, the celebrated portrait-painter and co-worker with the late Rosa Bonheur. The bridegroom was accompanied by Capt. Wm. Noble, J.P., of Forest Lodge, Uckfield.

It is said that before the end of 1901 the Orthodox, or Old Calendar, in Russia will be a thing of the past. The change was, of course, decided upon some time ago, and a Commission, under the presidency of the Grand Duke Constantine, has been sitting with a view to settling the matter.

The French postal authorities have decided to add to their postmarks the hour of the day at which the letter is forwarded. This is of itself no new thing to us, but the interest lies in the fact that the hours are to be numbered from 1 to 0 (midnight).

It is announced in the press that the astronomical instruments brought by the Germans from Pekin are now installed in the Orangery of the Sans Souci Park, Berlin.

The Cape papers report the establishment at Cape Town of the "South African Association for the Advancement of Science," to be carried on on the lines of the British Association. The preliminary meeting was held under the chairmanship of Sir David Gill.

Messrs. Cassell & Co., Ltd., announce a forthcoming work by Sir Robert Ball, 'The Earth's Beginning.'

The next Meeting of the Royal Astronomical Society will be on Friday, November 8, at 5 o'clock; of the British Astronomical Association on Wednesday, November 27.

From an Oxford Note-Book.

> But against one sublime department of science I would beg to enter a respectful warning. I do so with some apprehension, because I have distributed a prize in that department of science to-night. It is dangerous, in my judgment, to study astronomy, for astronomy kills ambition. What mind, after contemplating the eternal procession of unnumbered worlds, perhaps with their infinite generations of life, their various splendours, their history, their endless rolls of celebrity, their separate myriads of heroes, can return without a disheartening sense of the pitifulness and futility of everything on his own narrow universe?

Such were the words of no less a person than Lord Rosebery in distributing the prizes at Birmingham on October 15 last. One wonders how the main conclusion, that astronomy kills ambition, was arrived at: by experience or by observation? Has Lord Rosebery been studying astronomy himself and returned from his studies with a "disheartening sense of the pitifulness and futility" of the solitary furrow he has got to plough? Or is it merely that he has lately made the acquaintance of some astronomer and found to his bewilderment that the said astronomer has "no ambition"—either to win the Derby or be the Prime Minister or—(I forget the third thing)? Perhaps, however, the noble lord was really desirous of giving an encouragement to the study, and artfully disguised his real object under the warning he trusted human nature to disobey. This is the view of his words taken by our genial friend *Punch*, of date October 23. An imaginary exchange of telegrams is set going between two exalted personages who have read the speech, and are thereby stimulated to begin astronomical enquiries forthwith:—

> What does "planet in opposition" mean? Sounds like HARCOURT.——Telegraphed to LANSDOWNE, "Where is Nebular Hypothesis?" He replied, "Why worry about policy in Persia or China? We have none."——Went over to Nice and called on the Préfet. Always thought him a charming fellow. I said, "*Avez-vous appris astronomie? Avez-vous vu le Grand Ours?*" "*Pardon,*" said he. "*Le Grand Ours,*" said I, "*dans le nord?*" "*Ah, monsieur le marquis,*" said he, "*quelle charmante plaisanterie! Vous parlez du Tsar. Non, je ne l'ai pas vu.*"

And so forth. The Astronomer Royal may be grateful for the prudent reflection which prevented him being bothered with enquiries:—"Most injudicious reveal our ignorance to expert."

Contemplation of the "eternal procession of unnumbered worlds," whether it lead to the killing of ambition or no, is in any case not indulged in to excess by the astronomer, who is generally far too busy. However, in regarding astronomical study as being largely made up of contemplation, Lord Rosebery is only repeating a very common misapprehension. Adopting it as the true view for a moment, it is worthy of note that the effect of contemplating the stars has been very differently estimated by others. The following is an extract from the 'Matabele Campaign' by R. S. S. Baden-Powell: it struck me as pretty good for a soldier:—

> And then you take a last look at the glorious star-spangled ceiling overhead, and, until all is blurred in sleep, you see in the dark mantle above you the veil of ignorance that shrouds the Earth from heaven's light beyond: the starry points of brightness that tend to light us are holes made in that covering by the work of good men, whose example and whose teaching encourage us to try and take our little part in letting in the light in imitation of the greater radiant orb—to lighten up the darkness till the daylight dawns.

IN the early part of this number a report is given of the address given by Sir D. Gill on the occasion of the unveiling of the McClean gift. The ceremony excited considerable interest, and inspired a very pleasantly written editorial in the *Cape Argus*, which I venture to reproduce here *in extenso*, as I feel sure others will like to read it:—

THE MCCLEAN GIFT.

> The pleasant little ceremony at the Royal Observatory on Thursday deserves more than passing mention. It gives an opportunity for the cultivation of a virtue which is not too common at the Cape—the virtue of gratitude, which Shakespeare knew as a "noble thankfulness"; and if, for the nonce, the public should be led to depart from its usual Philistine attitude towards pure science and the higher walks of research, the change may not be ungraceful, and may do it good. The value of Mr. McClean's gift it would be hard to overestimate. In mere money's worth it was princely—more than the Imperial Government could well spare, and more than the Colonial Government could venture to dream of as an encouragement to unapplied science. It was given, too, at the right time and to the right place, there being immediate need for a wide development of spectroscopic work, and the Southern Hemisphere being poorly supplied with astronomical equipment compared with the affluent North.

Further, it did not come from a mere millionaire, willing to be moderately fleeced in return for a little notoriety; Mr. McClean was a skilled and assiduous worker in this branch of science, and what he gave was the outcome of a pure heart and a noble enthusiasm. Nor did he stop at the purely material gift, but gave time and thought and trouble to make sure that the telescope and its accompaniments should be fit for the performance of the very best type of work. He came to the Cape and resided for months here, and those who were privileged to meet him will always remember his unassuming ways and his unflagging interest in his work. He had a double purpose in coming, and by far the greater portion of his time was spent in obtaining the spectra of certain southern stars, in order to supplement his similar work in the North. When this was done, the kindly English gentleman left as quietly as he came. The scientific equipment of the Colony had been handsomely enriched by him, but so far as the general public was concerned he left "unhonoured and unsung." The gathering of yesterday was, then, most amply justified, and one could but wish that the "three cheers" called for by H.M. Astronomer might by some scientific contraption reach the pious founder at his home in Kent. The need of scientific and literary endowments at the Cape is well-known, and the forgetfulness of those whom the land has made wealthy is occasionally bewailed: it would ill become us, therefore, to be equally forgetful of the far-seeing liberality of a stranger who owed us nothing.

SOME little time ago I yielded to the entreaties of a press-cutting agency, and signified my willingness to receive any cuttings on astronomical topics. Almost immediately afterwards, Dr. Anderson discovered Nova Persei and I was deluged with press-comments of all kinds—a drawer full of them, which were submitted the other day to an interested correspondent. He wrote back as follows:—

> Please accept my best thanks for the newspaper cuttings. Not a little instruction can be gained from them, but also, alas! not a little amusement: for although the result is satisfactory when astronomers are allowed to speak for themselves, it is not always so with the editorial summaries and comments. It is, for example, somewhat disconcerting to read of "this new Comet in Perseus"; and to find Nova Aurigæ cropping up in two cuttings as Nova Rigue. Had old Tycho, too, been alive today he would have been astonished to read about the New Star of 1572 that "it was not really a new star, for it was known before as a sixteenth magnitude star." However, Editors have nowadays to know so much about what is happening on the terrestrial sphere that we can excuse a little ignorance about the celestial one.

AN examiner of my acquaintance has kindly sent me the following extract "for inclusion in my museum"; it occurs in the answers to a paper on 'Paley's Natural' (*sic!*):—

> The eye is a wonderful structure—this alone is sufficient proof to show to atheists that there is a contriver at the back, and that contriver is God. The eye is worked on the same lines as a telescope. The humours of the eye answer to the lens of the telescope, but there being a defect in the telescope owing to prismatic rays, they copied the eye by putting refractory powers. Now the eye is superior to the telescope in this.

Another candidate did an interesting thing, though his exploit had little to do with astronomy. The answers (to a paper on Political Economy this time) were to be written in books on one side of the paper only. "No notice would be taken," so the stern regulations gave warning, "of anything written on the left-hand page." To test the working of this rule, I suppose, the examinee wrote on the proper page a long and correct answer about banking.

On the page of which no notice was to be taken he wrote in large letters:—"N.B. The first bank in existence started by Noah, who floated a limited company when the rest of the Earth was in liquidation."

In glancing through a political history the other day (Goldwin Smith's 'United Kingdom,' vol. i. p. 515), the following sentence seemed worthy of remark, as making accurate use of a scientific metaphor:—"Here the tide of change reached high-water mark. After this the waves rose much higher, but not the tide."

From the *Morning Post*:—

THE POWER OF GRAVITATION.

TO THE EDITOR OF THE MORNING POST.

Sir,—Will you allow me to suggest to the president and other members of the British Association who spoke at Glasgow the other day that the attractive and repelling power is an inherent power in the particles themselves. What more simple, what more effective? This flashed on me fifty years ago, and all was clear.

Yours, &c.,

Sept. 30. J. B.

From *Truth* for Sept. 19:—

Dearest Amy,—What a wonderful season of glorious weather we have had, and are still having. These September days would be quite perfect here by the sea if only the darkness did not come so soon. And yet the very darkness is beautiful. There is a bright star to the south which makes a moonlight glimmer on the sea, so large is it, so near, and so brilliant. Is it Mars? Do get some one to tell me. It sets in thick clouds about nine o'clock.

Sent by a correspondent, who does not mention the source from which it was taken: the italics are his:—

Gesellschafts-Reise . .
. . . von Triest nach Ostasien

Unter Führung des Direktors der Manora-Sternwarte in Lussinpiccolo gedenkt sich eine kleine Gesellschaft dieser Sternwarte (2 Herren und 2 Damen) am 7. Februar 1902 auf einem Lloyddampfer in Triest einzuschiffen, um nach einem Besuche von

Egypten, Arabien, Indien, Beludschistan, Ceylon und der malayischen Halbinsel

China und Japan

zu bereisen. Die Rückkehr soll am 7. Juni 1902 in Fiume, bezw. 8. Juni in Triest erfolgen.

Distinguierte **Personen können sich an dieser Reise beteillgen.** *Gewinnabsicht besteht nicht.*

Die Kosten werden aus einer gemeinschaftlichen Reisekasse bestritten, in die jeder Theilnehmer pro Tag 40 Kronen ö. W. = 34 Mark für die 122tägige Reise also 4148 Mark einzuzahlen hat. *Die Sternwarte leistet Garantie.*

Anfragen sind unter Angabe des Standes zu richten an die

Manora-Sternwarte in Lussinpiccolo.

No. 312.] DECEMBER, 1901. [*Price* 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1901 November 8, at 5 P.M.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. 191 presents have been received since the last Meeting, including amongst others:—Tycho Brahe's 'De Nova Stella' (Facsimile Edition), presented by the Royal Academy of Sciences, Copenhagen; 'Harvard Annals,' vol. 28, pt. 2 (Spectra of Bright Southern Stars), presented by the Observatory; 'Lick Observatory Publications,' vol. 5 (Hussey's measures of double stars), presented by the Observatory; Observations of the Total Solar Eclipse of Jan. 22, 1898, presented by Sir N. Lockyer; 'Atlas photographique de la Lune' par MM. Loewy et Puiseux, presented by the Paris Observatory; 'Carte photographique du Ciel' (22 Charts), presented by the Paris Observatory; 'Annals of the Astrophysical Observatory of the Smithsonian Institution,' vol. 1, presented by the Institution; and Photographs of the Great Comet of 1901, presented by the Perth Observatory, Western Australia.

The President. I should mention to the Fellows of the Society that on the 31st October a joint Meeting of the Society and of the Royal Society was held to receive the preliminary reports of the expeditions which had observed the eclipse of last May. Most interesting communications, illustrated by lantern-slides, were made by Mr. Newall and Mr. Dyson who have been to Sumatra, and by Mr. Maunder who has been to Mauritius. I congratulate those gentlemen upon the value of the results obtained under not too favourable atmospheric conditions, and I think

I may say that most of us were agreeably surprised by the results being more satisfactory than we were led to suppose, knowing what the state of the weather had been. In consequence of these reports having been made to this Joint Committee, the matter will, of course, not come before this Meeting to-night in any form.

Mr. S. A. Saunder (presenting a paper on Measurement of Lunar Photographs). In a previous paper (*Monthly Notices*, vol. lx. p. 174) I called attention to the great uncertainty that attaches to our knowledge of the positions of lunar formations, and showed that much more accurate places might be obtained by measurement from Mösting A at the telescope, or on the excellent photographs now being taken at the Paris, Lick, and Yerkes Observatories. In continuation of my work on two of the Paris photographs described in my last paper, taken one a little after first quarter and the other a little before third quarter, I endeavoured to obtain absolute values for the eight constants (four for each plate) by an analytic method analogous to the combination of the two pictures in a stereoscope; but although 74 conditional equations were obtained from the measures of 37 points on each plate, the normal equations were practically indeterminate, and only enabled me to express the constants of either plate in terms of those of the other, thus reducing the eight unknown quantities to four. I then had recourse to the telescope and carefully measured the positions of 19 points relatively to Mösting A, obtaining their co-ordinates with a probable error of about 0″·1 in each. I used a filar micrometer, which limited me to distances not much exceeding 500″ from Mösting A. Two of these points were most kindly also measured for me by Professor Barnard with the Yerkes 40-inch (*Monthly Notices*, vol. lx. p. 540). When the work was nearly completed I found that Dr. Franz had already published measures of eight points, all near the limb, made with the Königsberg heliometer. None of these were really well defined on my photographs, but I was able to make some use of six of them. By combining these telescopic measures with measures of the same points made on the photographs I was then able to form 72 conditional equations for the four unknown quantities, and the solution of these gave absolute values for the constants of both plates. With these I recomputed from the photographic measures the positions of those points which had been measured at the telescope, and found the mean discordance between a place found at the telescope and that obtained from a single photograph to be 0″·38. When I could obtain the mean place from measures on two photographs this discordance was reduced to 0″·32. In estimating the value of these results it must be remembered that the lunar formations are not stellar points, and that they have to be measured often under very puzzling conditions of illumination. After my paper was written Mr. Hinks very kindly called my attention to the first volume of the Breslau 'Mitteilungen,' copies of which had just been received in England, and in which Dr. Franz gives the positions of 150 formations obtained from the measures of five Lick photographs. In what I should consider the essentials his methods agree with mine. We have both found places at the telescope by measurement from Mösting A, and have used these for obtaining the constants of the plates, thus rendering our work entirely independent of direct measures of the limb. In the details, however, there are many points of difference. In addition to Mösting A and the points already referred to there are 17 formations which we have both measured, and on comparing the two sets of results I find a mean divergence of $0''.39$ in the positions, which includes an apparently systematic difference of about $0''.2$ in the ordinates. This mean divergence is less than one-thirteenth of the mean divergence between those positions found as points of the first order by both Lohrmann and Mädler, whilst the systematic difference is about one-fifteenth of that between the same observers. It is a very great gratification to me to find my work in such close agreement with that of one who has given more attention to selenographic measurements than any other living astronomer, and to whom we owe the best determinations both of the physical libration and of the figure of the Moon. But the opportunities now open to us are not limited to the increase of accuracy in a comparatively small number of points, it is also possible to enormously increase the number of accurately measured points. Beer and Mädler's map was founded on 105 points of the first order, Schmidt's on 157. By measures on the photographs it will, I believe, be possible to raise this number to something like 5000, and these should open the way to researches in other directions.

For the photographs on which the measures here described have been made I am indebted to the generosity of M. Loewy. Copies were kindly made for me, with réseaux, by Mr. Bellamy, and the measures were made with an astrographic micrometer belonging to the Oxford University Observatory, kindly lent for the purpose by Professor Turner, to whom I am further indebted for a considerable amount of personal advice and assistance.

Prof. Turner. It is interesting to call attention to one point in connection with Mr. Saunder's work, and that is that it seems to be one of those fields of work not altogether suitable for large instruments. Mr. Saunder has been fortunate enough to secure the help of one of the very large instruments. Prof. Barnard learned on his last visit to England of some measurements that Mr. Saunder wanted, and he generously said that he would make them, but he found that the Moon was so bright with the Yerkes telescope that he had to give up such work after two or three nights. Sir David Gill also kindly promised to help with some measures made with the Cape heliometer; and I had a letter from him the other day, in which he says that he finds it extremely difficult to make these measures. He does not specify the exact difficulties *, but it is possibly another instance of a large telescope being not the best instrument for such work, and the smaller instrument, such as Mr. Saunder has himself been using, may be the more suitable one. In the large amount of work which he has not only already done, but still to do, it is well to know that there is a field for moderate instruments. Of course this does not apply to *photographic* large telescopes. The photographs taken with the large Paris equatorial coudé are indeed the mainstay of Mr. S. Saunder's work.

A vote of thanks was accorded to Mr. Saunder.

Rev. W. Sidgreaves. This contribution to the literature of the spectrum of Nova Persei 1901 consists of two papers presented as one, the second being added in the form of an appendix. The first paper was not ready for the June Meeting, but it was finished before any attempt was made on the star after its escape from the summer twilight; and it was then thought to be the last of the notes on the spectrum from the Stonyburst spectrograph.

On August 18 a trial exposure was made in order to see whether any of the lines were strong enough to impress the plate, and the result showed that, with a well-adjusted clock-rate for a slow trail, the spectrum might be photographed satisfactorily; and since then 24 photographs have been obtained on 17 nights. These will be discussed after a few remarks upon the first paper, which covers ground already gone over in 4 notes on the spectrum presented at the Meetings in March, April, and May.

The first observation to be made regards the displacement of the hydrogen lines. A reference to my Note 2 in April will show that I anticipated very different velocities from those which have resulted from the measurements made since that note was written. The dark lines show considerably less velocity, and the bright lines much greater velocity than I expected. The method of computation is that suggested by Orbinsky, but the spectrum of the comparison star (α Cygni) was not taken on the same plate. I am well aware of the seeming weakness of my structure, when we look at the slender foundations on which it is built. The measured intervals from $H\beta$ to the several hydrogen lines, including $H\mu$, are given in the original table, but not on the screen-projection. They range from no more than 7 to 21 mm. It will not therefore be a matter of surprise that there is so great a difference between the extreme results as 45 miles on each side of the mean velocity of 714 miles in the second. There are 10 determinations from the 10 separate intervals measurable on the plates of the Nova; and of these the five shorter intervals were measured on three plates, the sixth interval on two plates, and the remaining four longer lengths on one plate.

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* [*Note added Nov.* 21.—I find I have misunderstood the reason of the difficulties with the heliometer measures, which arises from the confusion caused by the superposition of images, and has already been referred to by Dr. Franz.—H. H. T.]

The bright-line velocity must appear to rest on a still weaker foundation, on account of the breadth of the lines; but for these measures many more plates could be used, and the breadth of the lines is not so great a drawback in this differential method as it is to the direct comparisons with terrestrial origins; for in the latter method everything depends upon the correct judgment of the position of the true hydrogen centre of the broad line, while in the former method the apparent scale-centres of the lines serve equally well, on the reasonable supposition that the several hydrogen lines are widened symmetrically.

It should be noted that the discordant figures opposite Hϵ in the table of bright-line velocity would be brought into agreement with the other figures if this line were taken to be the calcium line H. But as it stands it only pulls down the mean velocity from about 290 to 270 miles. This recession velocity of the bright hydrogen is too important in connection with any explanation of the star's original outburst to leave anything unsaid about it. It must be admitted that the mean results from both the bright lines and the dark lines may be vitiated by a systematic error, owing to inaccuracy of the scale-value of the unit of wave-lengths at the position of Hβ. A small error in this would seriously affect all the resulting velocities in the same way, though not to the same amount. But the adopted value is the least that could be assigned, and any true correction would be on the side of increasing the velocities. And further, we have an independent confirmation of the bright hydrogen velocity from a single photograph on February 28, by another prism, a direct compound prism. The constants of this spectrograph are, of course, quite different; they were, moreover, determined by another observer and by another method. The velocity given by this photograph is 310 miles.

The second observation to be made on the first paper regards the changing spectrum in March and April. In my Note 3 in the same April No. of the *Notices*, the spectrum is called the alternating spectrum, and its two forms are called the non-minimum and the minimum type spectrum, so closely the latter appeared to follow the minimum phase of the star's light-curve. In the table projected on the screen the two spectra are designated the *earlier* and *new* types; and you will see that the table shows conclusively that the new type spectrum is connected, not with any phase of the light-curve, but with an absolute magnitude of the star. All the earlier type spectra are found to be on dates when the star was brighter than 4·57 mag.; and all of the new type on days of less brightness; and the one photograph on April 9, when the star was at this magnitude, 4·57, has already been described in my Note 4 in May, as belonging partly to both types. It seems therefore that on this night the spectrum was caught in the process of changing.

The star's magnitudes are from the measures made at the Radcliffe Observatory, Oxford, and published in the *Notices* of April and May*.

Mr. Newall. Father Sidgreaves lays interesting views before us of these features in the spectrum of the Nova. I am glad to find that he comes to the conclusion that the very bright line near Hγ is not the hydrogen line. He certainly is to be congratulated upon his boldness in attacking the question of velocities in the way he has done, and also upon the consistency of the results deduced. He did not mention that there is another way of measuring the velocity, and it will be interesting to compare results. He gets these consistent results of about 700 miles a second from what he describes as the dark lines in the spectrum in each case. At the first Meeting of the Society at which observations of the Nova were dealt with, Mr. Seabroke revived a suggestion that the dark line which appears on the violet side of the bright line need not necessarily be the centre of the true dark line. I take it that Father Sidgreaves has measured the line which appears on the violet side of the bright line. Prof. Vogel, at Berlin, has measured these lines, and in his published account he inclines to the view that the *centre* of the dark line is not seen, but is bidden by the bright line overlapping it. The feature which Father Sidgreaves illustrates in his last slide, showing a great amount of detail in many of the bright lines, is of very great interest, and also those dealing with the alternation of two different spectra. It occurred to me when he was showing these results that probably the eclipse of one star by another would account for the phenomena quite as well as referring these changes in spectrum to a change in the temperature.

Mr. McClean showed some photographs of the spectrum of the Nova in corroboration of those shown by Father Sidgreaves.

Rev. E. Ledger. Might I ask Father Sidgreaves whether he considers that any resemblance between the spectrum of the Nova and of that of a gaseous nebula has increased or diminished of late ? †

Mr. A. R. Hinks read a paper, "Experimental Reduction of some Photographs of Eros made at Cambridge for the Determination of the Solar Parallax." The paper embodied the first results obtained with the new photographic equatorial of the Cambridge

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* Father Sidgreaves exhibited on the screen photographs of the spectrum taken on August 27 and September 5, which showed that the hydrogen lines were no longer the strong features of the spectrum. They appeared to be dying out as broad lines without thinning at the edges. The lines 386·9, 396·9, and 436·3 are each crossed by four brighter lines at the same spectral intervals, with the strongest of the four on the more refracted side. The line 436·3 is now a relatively strong line, and first appeared in the last week of March. The line 464·0 has faded. Lines 495·8 and 500·7 have appeared since February.

† *Note by Mr. Ledger.*—In *Astrophysical Journal,* July 1901, p. 83, Prof. E. C. Pickering says:—

"The resemblance to the nebula N.G.C. 3918 is now so close that in a

Observatory, and with the new measuring-machine recently described. The principal object of this experimental reduction was to test the working of a suggestion made by Mr. Cowell (*Observatory*, 1900 December) that the most convenient form of planetary ephemeris for astrographic work is a heliocentric ephemeris in rectangular coordinates.

The *measured* rectangular coordinates of the planet were reduced in the usual way to "standard for 1900·0." A heliocentric ephemeris of Eros was, with great kindness, computed for the purpose of the experiment by Prof. Millosevich. With this, and the ephemeris of the Sun from the *Berliner Jahrbuch*, it was shown that the *computed* standard coordinates of the planet, with an assumed value of the parallax, can be readily obtained, and nearly the whole of the work can be done on a calculating-machine. The material used consisted of fifty-two exposures made on 1900 November 9 and 10. In half the exposures guiding was by the stars; in the other half by the planet; and the two series were kept separate throughout. The *p. e.* of the deduced correction to the assumed solar parallax was in each case ±0″·02, and the *p. e.* of one equation of condition ±0″·096 and ±0″·088. The difference between the corrections to the parallax resulting from the two series was rather large; but evidence was given that the discrepancy should not, for the present, be taken as indicating a systematic error produced by the inevitable trail, either of stars or of planet, during the exposure.

The following conclusions were drawn from the present experimental reduction:—That the small value of the *p. e.* of an equation of condition gives hope of a good result from the photographic determination of the solar parallax; that it will be necessary to use eight-figure logarithms, and consequently the centesimal

---

photograph taken June 19, 1901, no marked difference was noted, except that the nebular line λ 5007 is about *eight* times as bright as Hβ in the nebula, and *only equal* to it in the *Nova*."

Then in *Ast. Nach.* No. 3741 (335), Father Sidgreaves says:—

"Line 500·7 μμ is *much stronger* than Hβ or Hγ, and of the same great width."

Prof. Pickering also says:—

"One line, λ 4364, is seen in the nebula but *not* in the *Nova*, perhaps owing to the proximity of Hγ."

While in the *Ast. Nach.*, Father Sidgreaves says:—

"Line 436·4 μμ is now a very prominent band, *considerably stronger* than its neighbour Hγ."

Note added subsequently by Father Sidgreaves, who heard the questions imperfectly.—The correct reply to Mr. Ledger's question is, I think, that the resemblance between the spectrum of the Nova and that of a gaseous nebula has increased in two of the lines and decreased in three. The likeness is better in the strength of the lines 500·7 and 436·4, and it is worse in the structure of the lines 436·4, 396·9, and 386·9.

system, in computing the ephemerides of the Sun and Eros; that the method of working in rectangular co-ordinates throughout is highly convenient; that it lends itself to the publication by all the co-operating observatories of their results in a form immediately available for incorporation in a final general solution; and that the micrometric measures might well be treated in a similar fashion.

The President. I am sure we must all congratulate Mr. Hinks on the first results he has got out, and they are none the less interesting in that they have involved mathematical calculations which are novel.

Prof. Turner. I also should like to offer Mr. Hinks my congratulation on these very fine results. Even his second result, which seems at first some way from the truth if we accept Dr. Gill's parallax as accurate, is not very far away from his other one. Considering the number of observations which will be ultimately available, I should think that errors such as are here shown will be swamped in the general mean result. Individual errors generally amount to something considerable, even in Cape work.

Mr. H. C. Plummer gave an explanation of his paper printed in the Supplementary Number of the *Monthly Notices* on M. Loewy's discussion of measures on astrographic plates, and also made some remarks descriptive of a model which he had made to illustrate some points in the mathematics of periodic orbits.

Mr. F. A. Bellamy showed lantern-slides illustrating his paper on the place of the variable star RU Herculis, deduced from photographs.

The following papers were announced and partly read:—

E. E. Barnard. "Recent Observations of the Position of Nova Aurigæ made with the 40-inch Telescope of the Yerkes Observatory."

S. A. Saunder. "The Determination of Selenographic Positions and the Measurement of Lunar Photographs: Second Paper."

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon for 1902."

T. C. Hudson. "A new Method of Interpolation."

H. C. Plummer. "On Periodic Orbits in the Neighbourhood of Centres of Libration."

Rev. W. Sidgreaves. "The Spectrum of Nova Persei from February 28 to April 26, 1901."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1902–3."

A. W. Roberts. "On the Variation of T Centauri."

A. M. W. Downing. "Comparisons of Geocentric Places of

Uranus, Neptune, and the Sun, calculated from Newcomb's Tables, with their Places calculated from Le Verrier's Tables, for 1904."

H. H. Turner. "Determination of Dr. Küstner's Magnitude Equation from Comparisons of his Meridian Observations in Zones +24° to +27°, with Measures of Photographic Plates taken at the University Observatory, Oxford."

F. A. Bellamy. "On the Place of the Variable RU Herculis and neighbouring Stars, from Photographic Measures."

A. R. Hinks. "First Reduction of Photographs of Eros made at Cambridge for the Determination of Solar Parallax."

Radcliffe Observatory, Oxford. "Further Observations of the New Star in Perseus."

W. H. Pickering. "Additional Note on the Green Flash at Sunset."

The following gentlemen were elected Associates of the Society:—

Prof. W. W. Campbell, Director of the Lick Observatory, San José, California, U.S.A.

Prof. J. Scheiner, Astrophysical Observatory, Potsdam, Germany.

M. Ch. Trépied, Director of the Observatory, Algiers.

The following gentlemen were elected Fellows of the Society:—

Spencer Lavington Fletcher, 38 Lammas Park Road, Ealing, W.

Prof. Monroe B. Snyder, Director of the Observatory, Philadelphia, Pa., U.S.A.

The following Candidates were proposed for election as Fellows of the Society:—

Ashton Charles Allen, 34 Kensington Palace Mansions, De Vere Gardens, W. (proposed by Rev. C. D. P. Davies).

John Hind Bell, Nautical Almanac Office, 3 Verulam Buildings, Gray's Inn, W.C. (proposed by A. M. W. Downing).

Walter Ernest Besley (H.M. Civil Service), 75 The Chase, Clapham Common, S.W. (proposed by F. W. Levander).

James Henry Booth, F.R.G.S., Schoolmaster, 32 Sunny Road, Southport (proposed by William Banks).

Arthur Thomas Cooper, A.M.Inst.C.E., Rivernook, De Montford Island, Reading (proposed by C. Hutchinson).

Joseph Alfred Hardcastle, 38 Eaton Square, S.W. (proposed by H. H. Turner).

Charles Sumner Howe, Ph.D., Professor of Astronomy, Case Observatory, Cleveland, Ohio, U.S.A. (proposed by Ambrose Swasey).

William Gylby Hutchinson, Rock Lodge, Roker, Sunderland (proposed by C. Hutchinson).

G. F. Johns, Perth Observatory, Western Australia (proposed by W. E. Cooke).

James William Lowber, M.A., Sc.D., Ph.D., LL.D., F.R.G.S., F.R.Met.Soc., 113 East 18th Street, Austin, Texas, U.S.A. (proposed by Capt. M. C. Hepworth).

H. Krauss Nield, Thorold Grove, Sale, Cheshire (proposed by Samuel Chatwood).

Capt. Michael Joseph O'Sullivan, Board of Trade Surveyor, 66 Fern Grove, Hartington Road, Sefton Park, Liverpool (proposed by H. G. Quirk).

Charles Tailent Spencer, A.M.Inst.C.E., La Quinta, Lyon Road, Harrow (proposed by A. G. Fowler).

Rev. Charles Lakeman Tweedale, M.A., The Vicarage, Weston, near Ottley (proposed by R. Wilding).

JOINT MEETING OF THE ROYAL AND ROYAL ASTRONOMICAL SOCIETIES.

In accordance with a convention made by the Eclipse Committee of the Royal and Royal Astronomical Societies, a Joint Meeting of the two Societies was held in the rooms of the former on October 31, at 4.30 P.M., to receive the Reports of the Observers sent out under the auspices of the Committee to observe the Total Solar Eclipse of May 18 last.

Sir William Huggins, President R.S., took the chair, *Dr. Glaisher*, President R.A.S., sat on his right, and the Secretaries of both Societies occupied seats on the dais. There was a full attendance of members of both Societies. After a few words explaining the reason of the meeting, Sir W. Huggins called upon the speakers in turn.

Mr. Newall showed photographs of the corona taken with the 4-inch photoheliograph with negative magnifier, which has been used in many recent eclipses in carrying out the programme of the Joint Permanent Eclipse Committee. He pointed out that the type of corona was that associated with the sun-spot minimum. The eastern equatorial region of the corona was unusually perturbed, and there were many prominences to be seen on the eastern limb of the Sun. Marked arches were also to be seen, especially in the S.E. quadrant, of strikingly symmetrical form, similar to those photographed by the Astronomer Royal in 1898. The western regions of the corona were more quiescent in appearance and there was in the last photograph, taken about 15 seconds before totality was over, a strong band of light close to the limb over a considerable arc of the limb, forming as it were a definite layer with comparatively sharp edge and similar in some respects to the appearances noted by Sir N. Lockyer last year.

He also exhibited a photograph of the "flash" spectrum taken with an objective-grating spectrograph, giving large dispersion in a short range of spectrum including little more than the region between the hydrogen lines Hγ and Hδ. Four photographs were obtained with this instrument: one of them shows the blue coronal ring at λ 4231; two others show the spectrum of the crescent before and after totality; and as each of these contains both bright and dark lines, they should be serviceable in the study of wave-lengths, &c. In one of the photographs more than 90 bright crescents have been counted in a range of spectrum of rather less than 100 tenth-metres.

He stated that he had examined the green coronal ring with a second objective-grating and could detect no sign of structure as of polar streamers.

Lastly he showed photographs of the corona taken with a polarizing camera. These exhibit marked polarization not only near the limb, but also remote from it. He stated that he had aimed at getting a series of photographs with related exposures, in order to study the proportion of polarized light in the corona at various distances from the Sun's limb. The value of the plates as a series was greatly diminished in consequence of the interference of cloud that partially obscured the corona throughout totality; for instance, the 8-seconds exposure shows less than the 2-seconds. But as each plate has two images on it, made simultaneously by two similar lenses, one provided with polarizing apparatus, the other without, material has at any rate been secured for the determination of the relative amounts of polarized and unpolarized light in definite regions of the corona. Mr. Newall referred to the generous assistance given by the Dutch authorities to those who had gone to Sumatra, and stated that he owed much to the efficient help rendered by Dr. L. Wallace, who had volunteered to come from Burma to Sumatra to assist in setting up the instruments and also in the observations during the eclipse.

Mr. Dyson stated that he and Mr. Atkinson had been more successful in Sumatra than they had expected from the large amount of cloud. The large-scale photographs were excellent and showed a large amount of detail in the corona, the most interesting feature being a triple arch round a large prominence. The small-scale photographs, covering a field of 15° round the Sun, showed a number of stars, the faintest being of the 6th magnitude, and established the fact that no intra-Mercurial planet as bright as $4^{m}\cdot0$ could have been within the field. Spectra of the "flash" were obtained at second contact, and extended, two spectroscopes used, from D_3 to λ 3296. The spectra of the corona showed a continuous spectrum reaching 3′ or 4′ from the limb. No absorption-lines are shown. The lines at λ 3384, 34 in the ultra-violet are strongly shown. The well-known lines are all shown, but no new lines. The photographs should give very fair determinations of wave-length.

Mr. Maunder said that his station had been fixed at the Royal Alfred Observatory, Mauritius, several photographs of which he showed on the screen. This Observatory was unfortunately planted in the most unhealthy part of the island, the site having been chosen before malaria had become endemic there. The unhealthiness of the site greatly interfered with the usefulness of the Observatory, and in the interests of science it was much to be desired that it should be removed to the higher ground in the centre of the island.

Mr. Maunder continued that his mission had been to act in concert with the Director of the Royal Alfred Observatory, Mr. T. F. Claxton, in the observation of the eclipse, and in consequence Mr. Claxton and he joined forces, and worked together as though they composed but a single observing party. Their united equipment included three cameras, each with an aperture of 4 inches, but giving images of the Moon respectively 8 inches, 2·4 inches, and 0·3 inch in diameter; the variation in scale being adopted in order to secure as far as possible a good representation of the different portions of the corona. Mrs. Maunder had also brought out an equipment of her own, the principal component of which was a 4¼-inch Cooke photo-visual telescope, kindly lent to her by Mr. G. T. Newbegin. She had communicated her report to the Joint Eclipse Committee, and he was permitted to refer to it now, as well as to his own. With these various instruments they had secured a large number of good photographs of the corona, the sky being cloudless at the time of totality, though the air was far from steady, and nothing like so transparent as in the eclipses of 1898 and 1900. Mr. Maunder showed several of the photographs on the screen, and drew particular attention to the crossing of the coronal rays in the N.E. streamer, as if one streamer were being viewed through another, and to a remarkable dark streak cutting across a number of north polar "plumes" in a slanting direction. Mr. Maunder also called attention to the success of an attempt to compare the general illumination at mid-totality with that during twilight after sundown.

Sir Norman Lockyer offered a few words of congratulation to the observers, who he thought had been very successful, remembering the comparatively bad sky which prevailed. *Dr. Glaisher* also complimented the observers, and the proceedings were brought to a close by a short speech to the same effect by *Sir William Huggins.*

THE BRITISH ASTRONOMICAL ASSOCIATION.

The Annual General Meeting of the British Astronomical Association was held on Wednesday, the 30th October, at Sion College, *Mr. G. M. Seabroke, F.R.A.S.* (President), in the Chair.

The Minutes of the previous annual meeting having been read and confirmed, the names of twelve candidates for membership were read and passed for suspension, and the election of two new Members by the Council was confirmed.

The scrutineers of the ballot for the election of the Council for the ensuing session reported that the following ladies and gentlemen had been duly elected:—*President:* Mr. G. M. Seabroke. *Vice-Presidents:* Rev. J. M. Bacon, Mrs. Walter Maunder, Dr. Isaac Roberts, and Mr. W. H. Wesley. *Treasurer:* Mr. W. H. Maw. *Librarian:* Mr. F. W. Levander. *Secretaries:* Messrs. A. C. D. Crommelin and J. G. Petrie. *Other Members of the Council:* Miss G. Bacon, Mr. G. F. Chambers, Mr. A. Fowler, Mr. W. Lynn, Mr. F. McClean, Mr. G. J. Newbegin, Miss M. A. Orr, Mr. S. A. Saunder, Mr. D. Smart, and Mr. C. T. Whitmell.

The Meeting unanimously confirmed the following appointments, which had been made by the Council: *Editor*, Mr. F. W. Levander. *Directors of the Observing Sections: Sun,* Rev. A. L. Cortie; *Moon,* Mr. W. Goodacre; *Mercury and Venus,* Mr. H. McEwen; *Mars,* Mr. E. M. Antoniadi; *Jupiter,* Mr. A. Cottam; *Saturn,* Mr. G. M. Seabroke; *Comets,* Mr. E. W. Maunder; *Meteors and Auroræ,* Mr. W. E. Besley; *Variable Stars,* Col. E. E. Markwick; *Photography,* Mr. R. Wilding; and *Zodiacal Light,* Mr. H. O. Barnard.

The report of the Council on the work of the eleventh session, together with the financial statement, were then presented, and were on the motion of *Mr. Holmes*, seconded by *Mr. Chatwood*, unanimously adopted.

Mr. Lynn proposed a vote of thanks to the retiring Members of the Council—Dr. Downing, Mr. J. Gore, Mr. A. Kennedy, Capt. Noble, and Mr. Thwaites—which was duly seconded and carried.

The auditors (Messrs. Henry Ellis and Gordon W. Miller) and the scrutineers of the ballot (Messrs. A. J. S. Adams, D. Avenell, and T. Clapton), and Mr. Tyson Crawford, who kindly lent his offices for the scrutiny, were accorded a vote of thanks.

The Rev. C. D. P. Davies moved, and *Mr. Hodge* seconded, a motion to delete, from No. 3 of the "Regulations for the Loan of Instruments," the Clause providing that borrowers "must have had previous experience with the class of instruments they propose to borrow," which was carried.

The President then moved, on behalf of the Council, the following addition to Rule XVIII., which provides for the management of the affairs of the Association by the Council:—"They may also make regulations for the management of the library and the collection of lantern-slides, the loan of instruments, and for other purposes, and afterwards alter and amend the same, such regulations, alterations, and amendments being subject to the approval of the Members of the Association in general meeting assembled. And such regulations shall be subject to alterations, or to be rescinded or added to, in the same manner as a rule or by-law of the Association." This was seconded by *Mr. Whitmell* and carried.

The President then delivered the annual address. He congratulated the Association on having laid a firm foundation and one that, so far as they could see, would uphold it indefinitely for years to come. Its numerical strength and the attendance at its meetings were well maintained. The work of the observing sections had been continued with the usual assiduity, and had resulted in reports from the sections relating to Mars, Meteors, Jupiter, and Saturn being issued during the past session. The dividing of the members into sections, under their several chiefs, had been a means of bringing together those interested specially in the several subjects, and giving an interest in the work which he thought would not otherwise be the case; and the fact that there were now 1153 members showed how many possessors of small instrumental means were interested in some branch of astronomy. The fact that there were so many branches of the science, all connected with one another and interwoven, made it of interest to minds differing much in lines of thought. There was room for the mathematician, the chemist, the physicist, and others, all of whom might follow their several special lines. Mr. Seabroke then proceeded to review the astronomy of the past year. He congratulated Sir David Gill and Prof. Kapteyn on the completion of the Cape 'Durchmusterung,' the last volume of which had appeared, forming a complete photographic survey of a portion of the Southern Hemisphere as far as the parallel of 19° South Declination, and containing the large total of 454,384 stars which had been measured on the plates and catalogued. He described briefly the work achieved during the Solar Eclipse, and related the circumstances in connection with Nova Persei. He then gave a lucid and valuable address on the present state of our knowledge of the ether, summing up the evidence for and against belief in its existence.

Mr. Maw proposed a hearty vote of thanks to the President for his interesting and suggestive address, which was accorded by acclamation.

The President, after briefly acknowledging the compliment, announced that as the next Meeting would be the 100th Meeting of the Association, it was proposed to hold a Conversazione, commencing at 8 o'clock, in place of the ordinary meeting, and that the December Meeting would be held on the 18th instead of the 25th of the month.

Mr. Crommelin read a note on Nova Persei, by *Col. Markwick*, and some notes by *Mr. Alexander Smith*, of Dalbeattie, on certain effects exhibited by Nova Persei, and several lantern-slides were shown in illustration. On some photographs taken by Mr. Smith in August and September, the image of the star was found to resemble, in general appearance, a circular sun-spot having a dark nucleus or umbra in the centre, which was surrounded by a lighter fringe or penumbra. Another negative (taken in March) showed that the image of the Nova had at that time the usual stellar aspect, a circumstance which indicated (whether the aureola had an optical origin or not) that important changes had taken place in the physical condition of the star during the interval. Mr. Smith had come to the conclusion that the aureola had no actual existence in nature, and from the evidence available it would seem to be due to some peculiar kind of luminous radiation of a different wave-length from those rays emanating from ordinary stars. The abnormal rays would not, therefore, focus at the same plane, and they consequently got an out-of-focus image. If this explanation was eventually proved to be correct, it was a circumstance of very great interest, and was quite unique in the history of stellar photography.

The President announced that the discussion on these notes and slides would be adjourned until the December Meeting.

THE ROYAL METEOROLOGICAL SOCIETY.

THE opening Meeting of this Society for the Session was held on Wednesday evening, the 20th November, at the Institution of Civil Engineers, *Mr. W. H. Dines, B.A.*, President, in the Chair.

A paper by *Mr. A. Lawrence Rotch*, on "The Exploration of the Atmosphere at Sea by means of Kites," was read by the *Secretary*. The author has for some years past devoted his attention to the use of kites to obtain meteorological observations at the Blue Hill Observatory, Mass., U.S.A., and he has successfully carried on the work of exploring the air there to a height of three miles by several hundred kite flights, executed in varied weather conditions, whenever the velocity of the wind exceeded twelve miles an hour. Certain types of weather, however, such as anti-cyclones, accompanied by light winds, can rarely be studied. Mr. Rotch now proposes the employment of kites carrying meteorographs on steamships, especially on vessels cruising in tropical oceans. He has himself demonstrated the practicability of this scheme, as on August 22nd last he raised a kite to an elevation of half a mile from a tow-boat in Massachusetts Bay, when the velocity of the wind at sea-level varied between six and ten miles an hour. At the end of the same month, when crossing the North Atlantic from Boston to Liverpool on the steamship 'Commonwealth,' he was able to raise kites carrying a meteorograph to an altitude of 1800 feet on five days out of the eight. The chief feature of these records was the rapid change of temperature with height.

A paper by *Prof. J. Milne, F.R.S.*, on "Meteorological Phenomena in Relation to Changes in the Vertical," was also read by the *Secretary*. When resident in Japan some years ago the author carried on numerous observations by seismographs for ascertaining changes in the vertical, and found that the more important displacements of the horizontal pendulums are of three types, namely, "intermediate," "long-" and "short-period" wanderings. During the last five years Prof. Milne has had continuous photographic records of a horizontal pendulum at his residence at Shide, Isle of Wight, and he now makes a comparison of these records with the weather-conditions prevailing during the first six months of 1901. He says that, assuming that a locality can be chosen where the diurnal wave and effects due to rain and desiccation are small, which his observations indicate as possible, records of what appear to be the effects due to barometrical gradients may be obtained. When these are large and appear suddenly the movements of the pendulum may be marked. At Shide the westerly displacement of a pendulum has for several years past been regarded as indicating the approach of bad weather.

The Determination of Jupiter's Mass from the Cape Observations.

AFTER Prof. Sampson's latest criticism * of my determination of the mass of Jupiter's system from the Cape heliometer observations, it seems desirable to discuss the points in question at a somewhat greater length than I did in my first reply, in order that no doubt may remain as to the extent to which my treatment of these observations was adequate and the results derived by me are to be trusted.

1. As to the first point, I hasten to confess that Marth has indeed neglected part of the perturbations in the mean distances. The term "mean distance" is somewhat vague. The constant part of the radius-vector consists of:—

I: The mean distance as computed by Kepler's third law pure and simple.
II: The correction due to the compression of Jupiter.
III: The correction due to perturbations of the first order by the Sun and the other satellites.
IV: The correction due to perturbations of higher orders.

What Laplace calls "mean distance" is I+II, to which he subsequently adds III as "perturbations" and neglects IV. Souillart takes I+II+III for the "mean distance" and adds IV as "perturbations." Now Marth has taken Laplace's definition of the "mean distance" (I+II) and Souillart's "perturbations" (IV), and consequently the perturbations of the first order (III) have dropped out. How this error escaped my notice is of little importance now; probably the explanation must be sought in the fact that there has been a long interval of time between the checking of the "mean distances" and of the "perturbations." But, whatever the origin of the mistake may be, I am greatly indebted to Prof. Sampson for pointing it out to me and thus enabling me to correct it.

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* See p. 376.

The neglected perturbations, expressed in the respective mean distances as units, are:—

$$-0{\cdot}000\,003, \quad +0{\cdot}000\,001^5, \quad +0{\cdot}000\,016, \quad +0{\cdot}000\,052.$$

These quantities should thus be added to Marth's expressions for the radii vectores (*M. N.* li. p. 514–15). The maximum geocentric effects are:—

$$(a) \;.\;.\;.\; 0''{\cdot}000, \quad 0''{\cdot}000, \quad +0''{\cdot}006, \quad +0''{\cdot}034.$$

It is, of course, easy, by means of the coefficients* $\frac{\partial s}{\partial a}$ and $\frac{\partial p}{\partial a}$, to compute the corrections to each tabular distance and position-angle, and to derive the resulting corrections to the right hand members of the normal equations. As my solution gives the unknowns in terms of these right hand members, the corresponding corrections to the unknowns may be found at once. As might have been expected, the position-angles are not affected at all. From the right hand members of the distance equations I find the following corrections to the values of the unknowns, to which I have added the probable errors as found in my paper:—

$$\begin{array}{llll}
\delta\mu = +0{\cdot}00382 & \pm0{\cdot}00188. & \delta\Delta h_1 = +0{\cdot}0002 & \pm0{\cdot}0150. \\
\delta z_G = -0{\cdot}0012 & 0{\cdot}0170. & \delta\Delta k_1 = +0{\cdot}0096 & 0{\cdot}0241. \\
\delta z_F = -0{\cdot}0007 & 0{\cdot}0289. & \delta\Delta h_2 = -0{\cdot}0017 & 0{\cdot}0097. \\
 & & \delta\Delta k_2 = +0{\cdot}0003 & 0{\cdot}0181. \\
\delta\Delta e_1 = +0{\cdot}00024 & \pm0{\cdot}00625. & \delta\Delta h_3 = +0{\cdot}00017 & 0{\cdot}00620. \\
\delta\Delta e_2 = +0{\cdot}00072 & 0{\cdot}00388. & \delta\Delta k_3 = +0{\cdot}00106 & 0{\cdot}00912. \\
\delta\Delta e_3 = +0{\cdot}00022 & 0{\cdot}00219. & \delta\Delta h_4 = -0{\cdot}00060 & 0{\cdot}00302. \\
\delta\Delta e_4 = +0{\cdot}00045 & 0{\cdot}00121. & \delta\Delta k_4 = -0{\cdot}00096 & 0{\cdot}00441.
\end{array}$$

$$\begin{array}{llll}
\delta x_1 = -0{\cdot}0050 & \pm0{\cdot}0211. & \delta y_2 = -0{\cdot}0004 & \pm0{\cdot}0502. \\
\delta x_2 = +0{\cdot}0025 & 0{\cdot}0156. & \delta y_3 = +0{\cdot}0014 & 0{\cdot}0181. \\
\delta x_3 = 0{\cdot}00000 & 0{\cdot}00783. & \delta y_4 = -0{\cdot}0021 & 0{\cdot}0251.
\end{array}$$

The unknowns Δp and Δq (*i. e.* the inclinations and nodes), which depend on the position-angles, are not affected.

It will be seen that, with the exception of $\delta\mu$, the corrections to all the unknowns are negligible compared with their probable errors, and are of a mere formal character. From the correction to μ we find the following correction to the reciprocal of the mass:—

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* These coefficients are not contained in my paper, but will be published, together with the other details of the observations and reductions, in vol. viii., part ii. of the 'Cape Annals.'

$$\delta M = \frac{3M}{100}\delta\mu = +0{\cdot}120.$$

The true value of M as derived from the Cape observations is thus

$$M = 1047{\cdot}346 \pm 0{\cdot}070,$$

which happens to be exactly identical with Newcomb's value, 1047·35 ±0·045 *.

2. "But" (I quote Prof. Sampson's words) "this is not the chief point of my objection to Marth's numbers." If I understand Prof. Sampson's meaning correctly, his chief point of objection to my work may be briefly stated thus:—

a. The adopted value of Jb^2 is outside the limits of possibility.

b. I ought to have determined a correction to Jb^2.

c. The influence of the uncertainty of Jb^2 on the derived correction to M is such that the latter correction is not reliable.

I will reply to these three points in succession.

a. Prof. Sampson objects to the value of Jb^2 (0·0247187), which was taken by Souillart from Damoiseau's MS. memoir. It would be interesting to know by what process Prof. Sampson derived from this value the ellipticity $\rho = \frac{1}{13{\cdot}492}$. Apparently the formula

$$(1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \rho = J + \tfrac{1}{2}\phi$$

was used. Now this formula, though theoretically correct (apart from the neglect of quantities of the order of ρ^2), is misleading in the practical application. Firstly, not the value of J, but that of Jb^2 is the quantity involved in the theory of the satellites, and the value of J itself, which must be used in the formula (1), consequently depends on the adopted value of b, or of the apparent equatorial diameter d of Jupiter's disc at mean distance (5·20280). Attention was called to this point by Cohn (*Ast. Nach.* 3404, pp. 327, 328) and by myself ('Discussion of Heliometer Observations, &c.' p. 83, footnote). Secondly, not ϕ, but the velocity ω of axial rotation is the quantity derived from the observations, and ϕ again depends on the adopted b or d. The formula (1) therefore contains implicitly the unknown diameter b, and can *not* be used to compute the value of ρ corresponding to a particular value of Jb^2, unless some assumption is made as to the value of b. This fact was pointed out by me (*l. c.* p. 84, footnote). If instead of (1) the more correct formula given in my paper (p. 84, formula 10) is used, and if in the terms of higher orders we take $\rho = \frac{1}{15}$, we have (see also Cohn, *l. c.* p. 330):

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* I regret having made a mistake in my paper by quoting the probable error of Newcomb's mass as ±0·065, this being the *mean* error.

$$(2) \quad . \quad . \quad . \quad . \rho = \frac{14}{15}\frac{\phi}{2} + \frac{14}{15} J \left\{ \frac{2}{3}\left(\frac{15}{14}\right)^3 + \frac{1}{3} \right\}.$$

The value of b implied by Souillart's values of the mean distances (Souillart, ii. p. 10) and the adopted value of M (1047·7) is $\log b = 6{\cdot}668410 - 10$, corresponding to $d = 36''{\cdot}951$. Jb^2, expressed in this value of b as unit, is 0·0247187. Thus, if we wish to adopt another value of d we must, in the formula (2), take

$$J = 0{\cdot}0247187 \left(\frac{36{\cdot}95}{d}\right)^2.$$

The value of ϕ is given by the formula

$$\phi = \frac{\omega^2 b^3}{f} M(1 + \Sigma m).$$

If we adopt $\omega = 2\pi/0^{d}{\cdot}41359$, we get for the values of ρ, corresponding to the apparent diameters

$$36''{\cdot}95, \quad 37''{\cdot}0, \quad 38''{\cdot}0, \quad 39''{\cdot}0,$$

respectively

$$1/14{\cdot}92, \quad 1/14{\cdot}90, \quad 1/14{\cdot}46, \quad 1/14{\cdot}00,$$

none of which is outside the range of possibility. The first of these values is, of course, the one implied by the date adopted by me. It is to be regretted that Prof. Sampson did not state which value of b (or d) he used. The simple statement that the formula (1) was used might easily give rise to the suspicion that Prof. Sampson, to continue his own metaphor, has actually stranded on a rock on which a warning lighthouse has been erected by Cohn and myself.

I may further remark that "if any of the different means collected by [me] had been adopted for ρ," and any acceptable value for b, and if then Jb^2 had been computed by the *correct* formula (2), I would certainly have found a value not differing very much from the adopted value 0·02472, which was presumably derived by Damoiseau from the motion of the node of the second satellite.

The quantity Jb^2 happens to be one of the very few elements of which more than one reliable determination exists. Laplace adopted $d = 39''{\cdot}00$ and $Jb^2 = 0{\cdot}02190$. If this value is reduced to the diameter $36''{\cdot}95$ it becomes 0·02438. Cohn finds from the motion of the node of the fifth satellite (also for $d = 39''{\cdot}00$) $Jb^2 = 0{\cdot}02095$ (*l. c.* p. 329), or 0·02332 for $d = 36''{\cdot}95$.

As a matter of fact I adopted Souillart's value, which is just as probable as any other, simply because tables based on this value had already been computed *.

b. Prof. Sampson apparently still thinks that it would have been possible to derive from the Cape observations "a better determination of J than Mr. de Sitter has allowed himself to expect." Suppose that the adopted value of J is in error by as much as *one fifth* part of its amount, *i. e.* nearly five times the difference between the determinations of Laplace and Cohn. The maximum geocentric effect of this error on the four mean distances would be

$$(\beta) \quad . \quad \pm 0''{\cdot}006^5, \quad \pm 0''{\cdot}004, \quad \pm 0''{\cdot}002^5, \quad \pm 0''{\cdot}001^5.$$

These quantities, and not those quoted by Prof. Sampson (*Observatory*, October, p. 378), which correspond to the *whole* of the quantity Jb^2, may be compared with those corresponding to the *correction* to M. We may also remark that, as the quantities (β) are referred to the *opposition*-distance † ($\log \Delta = 0{\cdot}6$), those relating to the correction to M should be given for the same distance, and not for Jupiter's *mean* distance ($\log \Delta = 0{\cdot}716237$), as was done by Prof. Sampson. They then become

$$(\gamma) \quad . \quad . \quad +0''{\cdot}023, \quad +0''{\cdot}037, \quad +0''{\cdot}058^5, \quad +0''{\cdot}103.$$

The comparison of the quantities (β) and (γ) would lead to quite a different conclusion from that reached by Prof. Sampson.

But this comparison seems to me to be entirely devoid of meaning. If I had happened to adopt Newcomb's mass instead of Kempf's, the quantities (γ) would all have been smaller than $0''{\cdot}001$; if, on the other hand, the adopted value of Jb^2 happens to be the true one, then the quantities (β) will vanish. As a matter of fact, the observer is not "called upon to perceive" the difference between the adopted and the true mean distances, but simply the apparent distances and position-angles between the satellites. Approximate values of the elements are adopted for the computation of tabular places, and the corrections to these adopted values are introduced as unknowns instead of the elements themselves, for no other purpose than to simplify the reductions by the substitution of linear differential formulas for the complicated rigorous equations which express the relation between the observed quantities and the elements of the systems. Whether the corrections found will be large or small simply depends on whether the adopted approximate values of the elements happen to differ much or little from the true ones.

[To be continued.]

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* Souillart, of course, overlooks the dependency of Jb^2 on b. If it had been my wish to adopt exactly Damoiseau's value, I should have reduced it from Damoiseau's diameter, $36''{\cdot}74$ (see Souillart, ii. p. 10), to my own, $36''{\cdot}95$. It would then become $0{\cdot}02442$. The difference is negligible.

† This gives a better representation of the real size of the quantities about which the discussion turns, as the observations were made near opposition.

Dr. Downing's Revision of Taylor's Madras Catalogue and the policy of Reducing Old Observations.

THE well-known Madras Catalogue of 11,015 stars for the epoch 1835, compiled by T. G. Taylor from observations made under his superintendence between the years 1831 and 1843, has long been regarded as a valuable authority in all questions which depend for their solution on the accurate determination of the positions of stars. Its usefulness was, however, greatly diminished by the existence of considerable errors, although Taylor had spared no pains to eliminate such errors before publication by every means in his power. The improvements in our knowledge of proper motion, and the more accurate determination of the constants of reduction, had made it clear to astronomers for some time past that a re-examination and re-discussion of these observations was desirable in the interests of astronomy of precision. The need for such a revision was recognized by Pogson, the late director of the Madras Observatory, who, in the first volume of the new series of Madras Observations, remarks of this catalogue:—"Its value at the present date may be inferred from the circumstance of a new edition being now called for by European astronomers. It is scarcely necessary to mention that I shall respond to this call with great pleasure as soon as the results of my own labours have been laid before the world, and time permits of the investigation of the remaining errors, both casual and systematic, which still require correcting in the former catalogue."

Unhappily Pogson's death, which occurred in the year 1891, prevented the fulfilment of this promise; but the work was taken up in 1893 by Dr. Downing, the Superintendent of the 'Nautical Almanac,' who is to be congratulated on the successful accomplishment of an arduous piece of work which constitutes an important contribution to astronomy. The expenses of the calculations in connection with this work have been met by a succession of grants from the Government Grant Committee of the Royal Society, while the cost of printing was defrayed, partly by a grant from the Publication Fund of the Royal Society, and partly by a special grant of £150 made by the Indian Office.

The revised catalogue contains the places of 10,988 stars, including the 7646 stars of Piazzi's Catalogue for 1800. The difference between this number and the 11,015 of the original Taylor is due to the fact that Taylor affixed numbers to a few stars not actually observed by him in either co-ordinate, while, in one or two cases, objects originally denoted by different numbers have been shown by the revision to be the same stars.

A marked improvement in the form of the catalogue has been effected by the introduction of columns containing the mean dates of observation, which were obtained from the books of original reduction deposited in the Record Room of Greenwich Observatory. The secular variation of the precession has been omitted, and columns containing the corresponding numbers in Bradley, Lacaille, and Piazzi have been added.

In the reduction of the R. A. the level corrections of the transit-instrument, as determined by Taylor, have been retained, but the azimuth has occasionally required comparatively large corrections. The clock-errors were determined from the adopted positions of standard stars taken from Auwers' Fundamental Catalogue, excluding stars north of +70° decl.

The principal corrections to the declinations were those due, (1) to Taylor's method of applying the corrections for errors of division of the circle, (2) to the difference between Atkinson's refractions with which the original observations were reduced and those of Bessel's *Tabulæ Regiomontanæ*, and (3) to the determination of index-error. Although Taylor had determined his index-error from the adopted positions of stars, yet in his nadir observations, combined with the known latitude of his observatory, there appeared to exist the material for making an independent determination of the declinations. A comparison of the index-errors as deduced from nadir observations with those obtained from the assumed places of stars exhibited, however, discordances, both of a constant and periodic nature, which induced Dr. Downing to abandon the nadir observations, and to base the declinations of the revised catalogue upon those of standard stars taken from Auwers' Fundamental Catalogue, between declinations +55° and −10°, applying the corrections, given on page 12 of that work, to reduce them to Auwers' mean system.

The observed right ascensions and declinations of standard stars were reduced to the beginning of the year of observation by the aid of the "Tabulæ quantitatum Besselianarum pro annis 1750–1864," while for other stars the corrections applicable to Taylor's Star-Corrections to reduce them to Star-Corrections as calculated with the Struve-Peters' constants were taken from specially constructed tables. The mean places were then reduced to the epoch of the catalogue with the Struve-Peters' precession constants without the application of any proper motion. In the case of a large catalogue such as Taylor's, where the majority of the proper motions rest on the slenderest foundation, this is probably the most satisfactory way of presenting the results.

A large number of errors of an arithmetical character were discovered and corrected in the course of the revision. Before publishing his catalogue Taylor had compared most of his places with Piazzi and Brisbane. Those places which he was unable to check by such a comparison contain the greater proportion of the large errors. As an indication of the thoroughness with which this part of Dr. Downing's work has been carried out, it may, perhaps, be mentioned that the late Mr. Stone had occasion to compare the places of nearly 4000 stars of his great Cape Catalogue with those of Taylor, and in the course of this comparison a number of large errors were found by him in the Madras Catalogue.

With one exception all of these have been independently discovered and set right in Dr. Downing's revision. The single exception is Taylor 2025, for which the precession in R.A. should be $+2^{\mathrm{s}}{\cdot}565$ instead of $+2^{\mathrm{s}}{\cdot}626$.

Many smaller errors have also been discovered and corrected in the revision, by which certain difficulties, experienced by Mr. Stone in several cases of suspected proper motion, have been removed.

The following are a few examples of proper motions deduced from a comparison of Stone's Catalogue with the original Taylor, and with Dr. Downing's revision, which illustrate the general tendency of the revision to reduce the larger proper motions:—

Proper Motions from a comparison of Taylor 1835 with Stone 1880.

No. in Stone.	No. in Taylor.	Original Taylor.		Revised Taylor.	
		R.A.	Decl.	R.A.	Decl.
		s.	″	s.	″
741	617	+0·047	−0·11	0·000	−0·06
3374	2819	+0·040	+0·01	−0·008	+0·02
6165	5056	−0·045	−0·04	−0·026	+0·01
6370	5241	+0·088	+0·01	−0·004	+0·10
6403	5270	−0·046	−0·03	−0·016	+0·01
6825	5616	+0·058	−0·05	−0·011	−0·03
8531	7303	−0·124	−0·11	−0·031	−0·05
8620	7367	−0·130	−0·11	−0·023	−0·04
8798	7507	−0·036	−0·04	−0·015	+0·02
9037	7703	−0·019	−0·03	−0·006	+0·02

It is of course impossible in such a large mass of computations to attain complete immunity from errors, and already Dr. Downing has had occasion to publish three short lists of *errata* (see *Monthly Notices* of the R.A.S. lxi. 8 & 9, and *Ast. Nach.* No. 3746); but in view of the fact that the original Catalogue has been in the hands of astronomers for more than half a century, and subjected to the minutest criticism during that period, the small number of these *errata* is the best evidence of the scrupulous care with which the work has been done, and of the degree of reliance which may be placed upon the general accuracy of the revised catalogue.

All this is well, and Dr. Downing has deserved the gratitude of astronomers for this important addition to their resources. He has afforded another illustration of the advantage arising from a careful re-discussion of the immense stores of material accumulated by the labours of our predecessors, the value of which had hitherto been impaired by a defective mode of reduction. Notable instances of the profit to be derived from this policy is afforded by Prof.

Auwers' new reduction of Bradley's and of Mayer's observations. Prof. Herman Davis is also engaged at present on a revision of Piazzi's Catalogue for 1800. At the Royal Observatory, Greenwich, Groombridge's Catalogue is undergoing a revision at the hands of Messrs. Dyson and Thackeray; and Prof. Ristenpart, at the Prussian Academy of Science, is occupied with the great work of collating all existing star-catalogues, to the number of 336, which will form an "Allgemeine Geschichte des Fixsternhimmels.' When these various labours are concluded we shall have made a considerable advance towards the ideal aim of one great department of astronomy, namely, the determination at frequent intervals of the relative positions of as large a number of stars as possible.

But it is perhaps to be feared that the very excellence of Bradley's work at Greenwich, and the accuracy of a few other great catalogues, may possibly have an injurious effect in leading us to depend too much upon these for the places of the standard stars, and in inducing us to make other catalogues of a differential character, even where the material exists for an independent determination of the positions of the equinox and equator. Dr. Downing has perhaps good reasons for discarding the thousands of observations of the Sun made by Taylor between the years 1831 and 1847, and for adopting the equinox of Auwers' Fundamental Catalogue instead of employing these observations for an independent determination. In the fourth volume of the Madras Observations Taylor discusses the observations of the Sun made between 1831 and 1837, and finds a correction of $-0^{s}\cdot144$ to the equinox as determined by the stars of the Greenwich Catalogue of 1830 which he used as standards. It is hardly to be supposed that the equinox upon which Dr. Downing bases his R.A.s is affected by an error of such dimensions. It would, however, add very much to the stability of the structure upon which our knowledge of proper motions, precession, and the Sun's motion in space is founded if all such carefully and systematically conducted series of solar observations were completely discussed, and the older the observations the more valuable would such a discussion be.

The unsatisfactory character of our knowledge of the position of the equinox at a given date is well illustrated by a sentence in the introduction to the Greenwich Ten Year Catalogue for 1890. Although the mean equinox correction determined at Greenwich for the ten years 1887–1896 is found to be

$$+0^{s}\cdot047,$$

the Astronomer Royal writes: "no correction has been applied, as the uncertainty of the system of proper motions based on Bradley's equinox and of the observations of the Sun due to the personality of the observers leaves the determination of the equinox somewhat arbitrary."

Nor should the attention of astronomers be confined to observations already published. If at any of the older observatories systematic records in MS. exist, these older observations should also be investigated and, if of sufficient value, reduced and published. Not till that is effected shall we be in a position to say that we have exhausted all the material or done our duty towards those who have gone before us.

At the Radcliffe Observatory there is a wealth of such material, and I may perhaps be permitted to take this opportunity of again directing the attention of astronomers to the importance of utilizing it. For the best part of a century the record of about 200,000 observations has been lying among other MSS. in the library of this observatory, but the observations have never been rendered available for the use of astronomers. It is well known that, more than 30 years after Bradley's death, his Greenwich observations were given to the world by Drs. Hornsby and Robertson, who were successively Radcliffe Observers at Oxford. It is not, perhaps, so well known that while occupied in editing Bradley's work these two distinguished men were at the same time carrying on an elaborate and laborious series of observations themselves, with instruments similar, but, in some important respects, superior, to those which Bradley used at Greenwich, and accumulating a mass of observations which, as far as one can judge from an examination of the results of a single year, are in every way comparable in accuracy and value with those of Bradley. In a paper published in the *Monthly Notices* of the R.A.S. lx. 4, I have given some of the results derived from a reduction of the observations made in the year 1774, when Hornsby began his work—that is to say, within 12 years of Bradley's death,—from which a judgment may be formed of the quality of these old observations as a whole.

The probable errors of a single observation of Bradley's as computed by Bessel in the *Fundamenta Astronomiæ* are

$$\pm 0^{\mathrm{s}}{\cdot}142 \text{ and } \pm 1''{\cdot}00$$

respectively. The corresponding quantities for Hornsby's work in 1774 are

$$\pm 0^{\mathrm{s}}{\cdot}053 \text{ and } \pm 1''{\cdot}00\,[*].$$

The similar quantities computed by Dr. Downing for the revised Madras Catalogue from several stars culminating near the zenith of Madras, and therefore rather favourably situated, are

$$\pm 0^{\mathrm{s}}{\cdot}081 \text{ and } \pm 0''{\cdot}60.$$

Thus it appears that, judged by their probable errors, Hornsby's observations are, in declination, of about equal accuracy with those of Bradley, while in R.A. they are considerably better. Judged by the same standard they are, in R.A., superior also to those of Taylor, which correspond to a date 60 years later.

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[*] In my paper in the *Monthly Notices* the probable error in declination was stated to be $\pm 1''{\cdot}27$. This was computed only from those stars which Bessel had used. The value given above is computed from *all* the stars between the zenith and equator contained in the paper.

These old records contain a very large number of observations of the Moon and major planets, and about 8000 observations of the Sun, a number but little less than that on which Le Verrier's tables were founded, and covering the period when the corrections to the mean longitude of the Sun as deduced from the observations of Greenwich, Paris, and Koenigsberg are most discordant, and when therefore independent observations are most desirable.

Are astronomers content to leave all this material unused, to moulder in oblivion, while more recent and possibly less accurate observations are submitted to a costly and laborious re-reduction? In his Report on the Progress of Astronomy *, Airy wrote in 1832: "We have done nothing in examining the past state of the heavens, or making it subservient to a knowledge of their future state: the reduction of Bradley's observations was left to a foreigner; the formation of Tables of the Sun and Moon, from British observations, even when the theory was put in a distinct shape, was left to foreigners; and as if we had determined to leave the present state of the heavens also in obscurity, our own observations have too generally been cast on the world unreduced, with a hope, I suppose, that others would have the zeal to reduce them." Can it be said that this reproach is even yet wholly removed while these observations remain unreduced? Since the date of Airy's Report a foreign astronomer has added lustre to a great reputation by his re-reduction of Bradley's observations. Another foreigner has voluntarily offered to complete Main's Radcliffe Catalogue, which was left unfinished at his death. Are we indifferent, in England, to such important and fundamental work?

The International Photographic Survey will hand down to posterity a record of the state of the heavens in our own day with splendid completeness. But of the past—those years for which we are wholly dependent upon the laboriously acquired observations of our predecessors—have we yet done all we can to place our knowledge on a thoroughly satisfactory basis? As the old Persian astronomer-poet Omar Khayyám said,

"The Moving Finger writes; and, having writ,
Moves on: nor all your Piety nor Wit
Shall lure it back to cancel half a line,
Nor all your Tears wash out a Word of it."

We ought, at least, to take care not to overlook nor disregard any information which may have come down to us from those irrecoverable years.

Radcliffe Observatory, Oxford,
1901, Nov. 9.

ARTHUR A. RAMBAUT.

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* British Association Report, 1832, p. 182.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Motion in the Line of Sight.

GENTLEMEN,—

It is always with extreme reluctance that I write on any personal matter. In Sir David Gill's recent Address at the Cape, printed in your last number, the statement appears that about 1865 my "attention was directed by Clerk Maxwell to this possibility of the new astronomy" (Determination of motion in the line of sight).

So far from this having been the case, the method suggested itself to me directly from Doppler's work, some time in 1862–1863. Among the first words of my paper on the subject, in *Phil. Trans.* 1868, are the following:—"We were fully aware at the time (1863)......that these comparisons might serve to tell us something of the motions of the stars relatively to our system" (p. 529).

The inclusion of Clerk Maxwell's letter in my paper came about in this way. Wishing to make the historical introduction to my paper as complete as possible, I asked my friend Clerk Maxwell, in 1867, to give me an account of some experiments which I had heard he had been making to detect the influence of motion on the refrangibility of light. His letter, which I did not receive until June 1867, appeared to me to be of so much interest that, instead of making extracts from it, I requested his permission to print it in full in my paper. Clerk Maxwell's reply, which I quote from a letter dated March 23, 1868, shows clearly that my work had been independent, and not undertaken in consequence of a suggestion of his. His words are:—"If it appears to you that what I sent you last summer would answer as part of your paper, it would be very agreeable to me to have it placed beside your work, so that if it should contain anything not applicable to your methods, or to which your methods are not applicable, the difference may be seen to be the effect of independent working, and not of opposition or criticism."

Yours faithfully,
WILLIAM HUGGINS.

Upper Tulse Hill,
1901, Nov. 9.

The Slow-pathed Aquarid Shooting-star of August 10th, 1901.

GENTLEMEN,—

It has just now been pointed out to me, by Mr. Besley, that the slow-moving shooting-star of August 10th last, 10^h 58^m, described in my letter in the October number of the *Observatory* as having been recorded here and at Farnborough, in Hampshire, was also well observed by Mr. Denning at Bristol. In a list of meteor-tracks seen by him on that date, with which Mr. Denning was so good as to provide me, but where this corresponding observation has, unfortunately, until just now escaped my notice, it was thus described:—$10^h\ 59^m$, 3rd magnitude, 10° in 1·4 second, from 5° +26° to 10° +35°; very slow; with train; probable radiant, 356° +4°.

Produced backwards this apparent path-line meets those observed at Farnborough and at Slough, respectively, at 351° −7° and 349½° −11°, some way before the gradual approach to, and intersection with each other backwards of the two latter paths at about 355° −19°, or 356° −20°. As was to be expected from the nearness of the Slough and Farnborough stations to each other, the latter crossing-point could only be regarded as a fair approximation to the meteor's real perspective radiant-point, and the new path-evidence supplied by Mr. Denning's observation of the meteor's course at Bristol (80–85 miles west from Farnborough and Slough) fortunately reduces this uncertainty, by its transverseness to both the other paths not very far from their conjunction, to a very narrow limit; for within the short triangle which Mr. Denning's path-line cuts off from the slender meeting-apex of the other two a point, near its base at 351° −11°, is only one degree distant from Mr. Denning's and from both the other two path-lines, and cannot, therefore, have been very far from the meteor's real radiant-point. Considering that these three observations were altogether independent, it is indeed remarkable that a point can be found, so far back as this one is from the recorded tracks, from which, to such a near approximation, they all three diverged, as nearly as possible as from a common centre.

As might also be expected from the shortness of the base-line used in the first calculation, the concluded real heights, path-length, and velocity were only moderately reliable. A single degree or two of error is considerable in a parallax of 14° or 15° which the first two observations furnished. But Mr. Denning's path-description clearly shows that quite this amount of inaccuracy had crept in, widening the relative distance apart of the paths about two degrees from the proper width of parallax which the recorded meteor-place at Bristol shows that they ought to have presented; and just as the great length of base empowered the Bristol path-account to pronounce quite positively on the radiant-point's improved position, so it also throws all this fault of parallax, and of the first calculated real height results, unquestionably on the two Farnborough and Slough observations, making it necessary to recompute the meteor's real path with corrected, better data. Approaching, therefore, those two latter paths one degree each towards each other, and taking all the three paths to have proceeded from the radiant-point 351° −11°, near ψ Aquarii (then 41° S. of E., alt. 14½°), I have now calculated an amended real path, and obtained, with the same facility as before, as respects the new observation's satisfactory agreement in direction of displacement with the two previous ones, the following more reliable results. Mr. Denning's apparent path of 10° was only one degree shorter at each end than corresponded with the two long paths at Slough and Farnborough, which are retained, however, in this new real path, as the meteor appeared nearly in those two places' zeniths, and may therefore probably have shown there more than could be seen at Bristol of at least the faint latter half of its flight. The described duration, 1·4 second, at Bristol might also, perhaps, have been a little longer if the whole length of path at Farnborough and Slough had there been fully visible.

Height at Beginning:—65 miles, over 2 miles north of Pyrford, near Woking, Surrey. Height at End:—57½ miles, over one mile south of Watlington, Oxfordshire. Length of Path, in 1·4, 1·4?, or 2–2·1 seconds (mean, 1·62 second):—30 or 31 miles. Real Velocity:—18¾ miles per second.

It would be difficult to deduce from the observations any much higher real speed than this, as a rather low estimate of the duration for the whole length of the meteor's course, as mapped at Slough, has here been combined with the largest recorded measure of its length of path; and also because the measures of real height, and length and speed, have all been increased to the full extents required by Mr. Denning's observation, without tasking that observation to bear any part itself of the errors which present themselves relatively among the three paths when they are all compared together. But the radiant-point's elongation from the apex of the Earth's way having been about 61½°, the meteor's parabolic-pathed theoretical speed of flight should have been 30¼ miles per second; and thus this Aquarid shooting-star was still, although not quite so abnormally sluggish in its real motion as in my former letter it was inferred to be from the first two observations, a very unmistakably and distinctly slow-pathed meteor.

I am, Gentlemen,
Yours most obediently,
A. S. Herschel.

Observatory House, Slough,
1901, October 25.

Joseph Beauchamp.

Gentlemen,—

"Science," as I once heard Dr. Gould emphatically and truthfully remark, "has no country;" but of course if a scientific man takes part in political or military concerns, he runs the ordinary risks of those who "take the sword." So astronomy lost, in early life, Gascoigne, whilst fighting on the royal side in our civil war, and she might have lost in later times Encke when he joined personally in the contest to free the fatherland from the tyranny of Napoleon. But I think we may drop a tear over the sad fate of Joseph Beauchamp, the hundredth anniversary of whose death takes place this month, and his many sufferings in the cause of our science almost justifies Lalande in calling him a martyr to astronomy. If we read the account of him by the latter in his 'Histoire abrégée de l'Astronomie depuis 1781 jusqu'à 1802,' we cannot help thinking of the remark about Ulysses in the Odyssey, translated by Pope "long exercised in woes." Beauchamp was born at Vesoul in 1752. His uncle Miroudeau was made bishop of Babylon in 1774, and desiring to have the assistance of his nephew, sent for him to Paris to prepare him for his duties. Young Beauchamp had an early taste for science, which was encouraged by Lalande, with whom he had become acquainted, and who took great interest in instructing him in making astronomical observations, which, he pointed out, would be very useful in the East, as a means of improving the knowledge of the geography of those countries and in other ways. Miroudeau and Beauchamp travelled to Aleppo together; neither was "master of the Tiger," but one went on to the Tigris. Miroudeau, finding that he could not travel farther, deputed his nephew to act as his vicar-general, in which capacity he proceeded to Bagdad. Lalande sent him some astronomical instruments which he had obtained from the Minister of Marine, and of these, he tells us, Beauchamp made excellent use. In 1784 he travelled to Bassora and to the Persian Gulf; whilst on an Arab boat there a calm at sea came on, and he was destitute of a drop of water in a tropical sea for forty-eight hours, and at last returned to Bassora, apparently in a dying state. On his recovery he went, at the instance of Lalande, to the Caspian, in order to rectify the geography of its coasts by astronomical observations; on the way he was assaulted and robbed, which brought on a fever that lasted for eighteen months. However, he obtained many valuable observations, particularly at Kasbin of an eclipse of the Moon, which took place on the 30th of June, 1787, and subsequently returned to Bagdad. He had observed a transit of Mercury on the 4th of May, 1786, and devoted very particular attention to that planet, obtaining, according to Lalande, more observations of it than had been made since the origin of astronomy, and observing its place in positions nearer to the Sun than had ever been done before. He also recorded the places of several stars which could not be seen at Paris, and some of these were used by Lalande as supplementary to those in the 'Histoire Céleste.' Towards the end of 1789 he left Bagdad, and arrived in Paris on the 3rd of September, 1790. Lalande could not succeed in procuring for him a commission to return to Bagdad and continue his observations, but in 1795 got him nominated by the National Convention as consul at Muscat in South-eastern Arabia. It was necessary to go to Constantinople first, and he proceeded thither through Italy, arriving on the 22nd of November, 1796. From there he went to Trebizond, and determined the geographical position of Sinope and other places on the coast of the Black Sea. He prepared himself to go to Muscat, but the war with England rendered the journey dangerous and useless. On the 24th of May, 1797, he wrote to Lalande, "S'il m'arrive malheur, vous vous souviendrez de mon dévouement pour vous et pour l'astronomie." But in 1798 he was sent to Egypt pending being able to proceed to Arabia, and at the end of that year offered his services to Napoleon to go on a diplomatic mission to Constantinople. The ship was taken by an English cruiser, and, under suspicion of being a spy, Beauchamp was immured in the Turkish fortress of Fanaraki, a few leagues to the north of Constantinople, on the shore of the Black Sea. Liberated at the peace nearly three years afterwards, he returned to France, but, soon after his arrival, died at Nice on the 19th of November, 1801, of a disorder of the chest which he had contracted during his captivity. This was little more than five years before the death of his constant friend and patron, Lalande, who made great use of his observations.

I noticed in reading Beauchamp's account of his travels in Persia (printed in the 'Journal des Savans') that he was somewhat disappointed with regard to the transparency of the skies in that country. He had been told, he says, that the stars did not scintillate there, but he noticed very distinct scintillation up to 45° of altitude. And he considered that the air was sometimes (though more seldom) as clear in Paris as in Persia.

Yours faithfully,
W. T. Lynn.

Blackheath, 1901, Nov. 6.

NOTES.

Comet Notes.—Three definitive orbits have recently been published, the first of Comet 1896 IV., by Henry A. Peck (*Ast. Journ.* 509). This was discovered by W. E. Sperra, of Randolph, Ohio, on 1896 Aug. 31. It was very faint and diffused, and was only in sight for a month, consequently the probable errors of the resulting orbit are large:—

			Prob. Error.
T	1896 July 10·9427 G.M.T.		±0·0047
ω	41° 3′ 11″	1896·0.	±31″
☊	150 59 16		±21
i	88 26 2		± 4
q	1·14289		±0·00014

It will be noticed that the plane of the orbit is very nearly perpendicular to the ecliptic.

The other orbit is that of Comet 1897 I., by Dr. Johannes Möller (*Ast. Nach.* Supplementary Publications, No. 2).

This comet was discovered by C. D. Perrine at Lick on 1896 Nov. 2, and was in sight for a long time, first in the northern, then in the southern hemisphere, the last observation being made at Cordoba on 1897 April 29. Owing to this long interval, and to the fact that for a considerable part of it the comet was an easy object with a well-defined nucleus, the resulting elements should be fairly trustworthy. But the manner in which weights have been assigned seems open to much criticism. In particular we would note that he assigns weight 1 to the Greenwich observations of R.A., but gives weight 0 to their observations of declination, which the residuals do not appear to justify. In fact the cross-bar micrometer used at Greenwich gives more reliable results in declination than in R.A.

We append the actual comparison of the Greenwich observations with the mean of those made at all the other observatories, which will show that, except on Nov. 30, the agreement is quite satisfactory, as cometary observations go:—

Date.	Error of ephemeris by Greenwich observations.	Error of ephemeris by mean of other observatories.
	″	″
1896 Nov. 4	−36	−40
Nov. 29	+ 6	+ 2
Nov. 30	+17	+ 1
Dec. 4	+ 5	+ 9

The residuals of the Greenwich observations are no greater than those of several observatories that have been used, and as this is not an isolated instance of the kind, it is worth while to draw attention to this arbitrary procedure.

The orbit proves to be distinctly hyperbolic, more so than that of any other comet whose elements are well determined, with the single exception of 1889, which has slightly greater eccentricity. Dr. Möller thinks that probably this result is due to planetary perturbations, and that the original orbit was parabolic or elliptic. He proposes to make some further researches on this point.

			Prob. Error.
T	1897 Feb. 8·14078 Berlin M.T.		±0·00085
ω	127° 18′ 59″	1897·0.	±3″·2
☊	86 28 31		±2·2
i	146 8 15		±0·9
log q	0·026444		±0·000002
log e	0·000402		±0·000015

The third orbit is that of Comet 1899 I., which Mr. C. J. Merfield investigates in *Ast. Nach.* 3747–8. This comet was discovered 1899 March 3 by Lewis Swift, and was observed till August 10 following. Almost 600 observations are available, including Greenwich photographs on 15 days. The resulting elements ought to be very reliable, but some difficulty is introduced by the physical complexity of the comet, the nucleus being eccentric and double, and great changes taking place in it, so that it was doubtful which point corresponded to the centre of mass.

Perturbations by Venus, Earth, Mars, Jupiter, Saturn have been applied. The definitive orbit is hyperbolic, as follows:—

Epoch of Osculation 1899 March 12.

$$\begin{array}{ll} T \ldots\ldots\ldots & 1899 \text{ April } 12{\cdot}97801 \text{ G.M.T.} \\ \left.\begin{array}{ll} \omega \ldots\ldots\ldots & 8^\circ\ 41'\ 46'' \\ \Omega \ldots\ldots\ldots & 25\ \ \ 0\ \ \ 0 \\ i \ldots\ldots\ldots & 146\ \ 15\ \ 30 \end{array}\right\} & 1900{\cdot}0. \\ \log q \ldots\ldots & 9{\cdot}513979 \\ \log e \ldots\ldots & 0{\cdot}000152 \end{array}$$

Prof. Hale writes that the Yerkes staff were in no way responsible for the erroneous announcement of the position of the great comet in April last, which appeared in the daily press. Prof. Frost, while sweeping the Sun's surroundings for a possible trace of the object, came across a suspicious-looking object north of the Sun, which for a time he thought might be it. But it was much too vague and doubtful to make any announcement. However, the matter happened to reach the ears of a newspaper reporter, who was on the premises making enquiries about the comet; with more zeal than discretion he hastened to put the erroneous position in print, and it was forthwith cabled over the world. A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered, all at Heidelberg:—

Provisional designation.	Date of discovery.	Discoverer.	Magnitude.
HC	1901 Nov. 2	Wolf, Kopff.	11·5
HD	Nov. 5	„ „	13·0
HE	Nov. 3	Kopff.	12·5
HF	Nov. 6	Wolf.	11
HH	Nov. 12	„	11·8
HI	Nov. 12	Carnera.	11·3
HK	Nov. 16	„	12

Planets designated as GW, HG proved not to be new, but identical with 247 Eukrate and 455 Bruchsalia respectively. HK may not improbably be identical with (367) Amicitia.

The following planets have received permanent numbers:—FV 464, FW 465, FX 466, FY 467, FZ 468, GB 469, GJ 470, GN 471, GP 472.

The planets FT, FU, GA, GC, GD, GE, GF, GG, GK, GL, GM, do not receive permanent numbers, not having been sufficiently observed.

Popular Astronomy for November contains an article on Eros, by Mary Clark Traylor, which we refer to in order to correct two misstatements:—

1. As regards the date of the last very favourable opposition, instead of 1893 December, read 1894 January 20. That was practically the closest approach possible, opposition and perihelion being coincident.

2. It is stated that there will be a very favourable opposition in Jan. 1917, the planet's distance being only 25,700,000 miles. This is not correct; the opposition of 1917 will be not at all a remarkably near one, the planet passing its perihelion 1916 Dec. 13, while the Earth does not pass the same longitude till 1917 Jan. 20. This is, in fact, a worse opposition than that of 1907, the planet then passing perihelion 1908 Feb. 24.

The correct date of the next favourable opposition is Jan. 1931; that and the one of 1938 are about equally favourable, but neither is so good as those of 1894, 1975. The period of 81 years, as the writer states, brings about an almost precise recurrence of the relative positions of the Earth and Eros. A. C. D. C.

NOVA PERSEI.—When the new star in Perseus was under observation last spring, it was remarked that its image on a photographic plate was unlike that of other stars, not having such a hard, well-defined outline, but being woolly and soft. Mr. Henry Ellis, of Potter's Bar, published some notes on the subject in the Journal of the Brit. Ast. Assoc., dated April 19, in which he discussed the appearance on photographs he had obtained with a portrait-lens and with a mirror. He made the suggestion that the appearance was due to the gaseous atmosphere surrounding the star and not to the fact that the plate was out of focus for the peculiar light emanating from the Nova, and added in support of this view that a plate taken with the reflector showed this soft image as well as those taken with the lens.

Shortly after the star became a convenient object for evening observation in August last, this effect became more pronounced, and about August 19 or 20 the image (we write specially of photographs taken with refracting telescopes) showed a definite halo and looked, as some one described it in the *English Mechanic*, like a small sun-spot with an umbra and penumbra. M. Antoniadi, who fortunately got a good photograph on August 19 showing this, published his results in *Knowledge* for November and gave it as his opinion that this was not the effect of a real nebulosity, but was due to the nature of the Nova's light, which, remembering the peculiarity shown by the spectrum, viz. that the lines in the ultra-violet were particularly strong, seemed not unlikely. Dr. Max Wolf, of Heidelberg, was also of this opinion.

The *Astrophysical Journal* for October contained much about the star. Prof. Barnard, after first pointing out that for a planetary nebula the focus of a large refracting telescope is about a quarter of an inch further from the object-glass than for a fixed star, went on to say that on Aug. 12 and Sept. 3 he found no appreciable difference in the position of focus of the 40-inch refractor for the nebula and for a 7·5 mag. star, and that he had "not been able with certainty to see any nebulosity." In this same number Mr. G. W. Ritchey, of the Yerkes Observatory, published a copy of a photograph taken with the two-foot reflector of the observatory on a Cramer "Crown" plate with three hours fifty minutes exposure, which shows Nova Persei surrounded by three or more large broken rings of nebulosity, but there is no trace of the aureola *close* round the star above mentioned. Prof. Barnard, after seeing the photograph, again examined the Nova and says, "Knowing the exact position of the various parts of the nebula, these were perhaps seen, but the night was not sufficiently pure to make certain of it."

The next points in the story are contained in three telegrams sent through the Kiel bureau, the first dated Nov. 11:—

"From Crossley photographs Perrine finds that four principal condensations faint nebula surrounding Nova Persei moved southeast one minute arc in six weeks."

It need only be added to this that the Crossley reflector is now at the Lick Observatory, and that Mr. Perrine is a very experienced observer.

The subsequent telegrams read thus:—

"*Nov.* 12.—Ritchey states that a photograph obtained at the Yerkes Observatory on November 9 confirms the large motion of the nebula near Nova."

"*Nov.* 13.—Ritchey finds the nebula surrounding the Nova probably expanding in all directions."

The parallax of Nova Persei is not yet known, but assuming a parallax of 1″ as a minimum limit of its distance, a motion of 1′ in six weeks corresponds to about 1500 miles per second.

DOUBLE STARS.—It will be remembered that Prof. Hussey suspected the period of δ Equulei to be about 5·7 years, and not 11·4 years, as generally accepted. In the portion of the orbit now being described it is almost impossible to decide the point for at least a couple of years by visual observations alone.

Special efforts have been made with the Mills spectrograph at Lick to secure spectrographic observations with the view of aiding in the solution. A few successful negatives have been taken. The spectra of the two components are clearly shown, with displacements corresponding to a relative motion at present of about 35 kilometres per second.

If the distance between the corresponding lines decreases and vanishes in the next few months, we shall have conclusive proof that the period of the star is in the vicinity of 5·7 years; and if otherwise, the longer period should have preference.

In the *Astronomical Journal*, no. 509, Dr. Chandler gives an investigation of the period of the variable star Algol.

We are glad to welcome the last Lick volume, which is devoted to Prof. Hussey's labours on the double stars discovered by Otto Struve at Poulkova. We hope to give our readers some idea of its scope in our next issue.

THE LEONID METEORS.—Watch was kept at Greenwich on Nov. 14, 15, and 16, and between 40 and 50 Leonids were seen after midnight on each of the first two nights. Mr. E. C. Willis, of Norwich, writing to *Nature* of Nov. 21, states that he saw altogether 202 Leonids on these three nights and 212 other meteors. Reports have appeared in the daily press that somewhat brilliant displays were seen in the United States on the nights of the 14th and 15th. An officer of a vessel reports having seen a great shower near Cape Hatteras early on the morning of Friday, November 15th.

THE SYMONS MEDAL.—The Council of the Royal Meteorological Society, at their Meeting on Wednesday the 20th inst., designated Dr. Alexander Buchan, F.R.S., as the first recipient of the Symons Gold Medal, in recognition of the valuable work he has done in connection with meteorological science.

This Medal, which is to be awarded biennially, was founded in memory of the late G. J. Symons, F.R.S., the distinguished meteorologist and originator of the British Rainfall Organization.

The Medal will be presented at the Annual Meeting of the Society on January 15 next.

THE ANNULAR SOLAR ECLIPSE 1901, Nov. 10.—A party of French observers under Count de la Baume Pluvinel went to Cairo to observe this phenomenon, which, judging from an account by M. Janssen in the *Comptes Rendus* (vol. cxxxiii. p. 768), they did with success. Count Pluvinel photographed the spectrum of the solar light grazing the Moon's limb, and the photographs showed no trace of absorption which might be caused by a lunar atmosphere.

THE COUNCIL OF THE ROYAL SOCIETY.—Mr. F. McClean was among those recommended by the President and Council to serve on the Council of the Royal Society for the ensuing year. But as Mr. McClean found that he would not be able to accept the honour, his place has been filled by Prof. H. H. Turner.

Knowledge is issuing for the second time a 'Diary and Scientific Handbook' for the coming year, which will combine astronomical ephemerides and information with the usual tables and pages found in diaries and some descriptive articles on astronomical and other scientific subjects.

The Editors regret that, owing to much pressure on the space at their command, they have been unable to deal with several interesting works which now lie on their table. They hope that these may receive adequate treatment in the coming year.

The next Meeting of the Royal Astronomical Society will be on Friday, December 13; of the British Astronomical Association on Wednesday, December 18.

From an Oxford Note-Book.

So the Reflex-Zenith Tube is not a failure, and Airy was right after all! Dr. S. C. Chandler has shown, by one of his beautiful investigations in the *Astronomical Journal* (No. 511), that "in the installation and persistent use of this instrument Airy was building better than he knew, and in an unforeseen way conferred on astronomy a higher benefit than his actual aim would have been had it been attained." And yet Airy's expectations were high enough, as may be gathered from his official reports of the period (see the Volumes of the Greenwich Observations for 1847 onwards). And they were shared by others. In presenting the R. A. S. gold medal to M. Peters in 1852 for his works on nutation and stellar parallax, the President (J. C. Adams) said:—

> We may hope that the beautiful reflex-zenith telescope of the Astronomer Royal, the magnificent heliometer which is in the able hands of Mr. Johnson, and the improved method of recording star-transits by means of galvanism, will enable us ere long to take many firm, though long-reaching, steps into regions of space hitherto untrodden.

Airy has related somewhere, though I have been unable to lay my hands on the reference, that the idea of the reflex-zenith tube occurred to him during a walk in the fields, and the simplicity and efficiency of it so tickled his fancy that he "burst out laughing": and he proceeded to set up the instrument with the utmost confidence. And yet

> Nothing in modern astronomy is so melancholy a record of futile result from a promising project pursued with admirable perseverance under disheartening circumstances, as the history of the Greenwich Reflex-Zenith Tube. All investigations of the observations made with it have been absolutely nugatory, and the memoirs communicating them have candidly confessed failure in arriving at any acceptable astronomical conclusions or any determinable reason therefor.

So writes Dr. Chandler in his paper above referred to. In fact the instrument was expected to give the absolute parallax of γ *Draconis* and the constant of aberration. The former came out sensibly negative and the latter hopelessly discordant from other determinations; and no reason for these anomalies could be assigned. Dr. Chandler has now shown that they are directly traceable to the Variation of Latitude; and that the observations when properly discussed confirm in a remarkable manner the law of Variation which he has propounded. He does not go on to find the correct values of parallax and aberration, as one might hope: possibly this will come later. But in any case he has rehabilitated these observations, as he has already done for Pond's observations. Pond was much attacked at one time. In the same President's Address from which I have just quoted we read also:—

> The celebrated controversy between Brinkley and Pond is discussed at considerable length and the labours of the latter astronomer are criticised with great severity. M. Peters considers that Brinkley was far superior to his opponent in his knowledge of the theory of his instruments and in the use of precautions to avoid error.

Nearly half a century later Dr. Chandler vindicated Pond's work, and now he has done the same for the work with the Reflex-Zenith Tube. Certainly Greenwich owes him a great debt: if there were only a *Liber Benefactorum* at the Observatory his name ought to be writ large in it.

On one point Dr. Chandler will perhaps allow a friendly correction, or rather an additional piece of information, for he could not be expected to know it. The "apparently hopeless undertaking" was not "abandoned in 1882 after thirty years' continuous trial:" on the contrary, a search was then begun for the source of error, on the initiative of the present Astronomer Royal. It was thought that there might be some temperature effect on the wire intervals, and to investigate this the micrometer was turned through 90° and furnished with a great number of wires for observing transits of γ *Draconis.*

After these had been made for several years the results were discussed, but gave no sensible indication of such an effect; and the instrument was then restored to its original arrangement and the normal observations resumed, as can be seen from the Astronomer Royal's annual reports. I do not think, however, that any of the recent observations have been published, though it may be hoped that they now will be. The unsatisfactory feature noticed by Dr. Chandler, that the observations are now only made from May to October, is, I fear, due to the deterioration of the Greenwich climate. We have looked for the star in the daytime very assiduously, but with poor success. Perhaps, now that the instrument is shown to be generally sound, another one may be set up in a better climate.

The sensation of the past month has been the photographing of what may prove to be the products of the *Nova Persei* explosion; and I humbly offer my hearty congratulations to the Yerkes and Lick Observatories (about priority I have seen no information) on the novel and wonderful results they have obtained. Our thanks are also due to Mr. Ritchey and the Editors of the *Astrophysical Journal* for the prompt publication of one of the photographs.

If the telegrams announcing the outward travelling of the nebulous matter from *Nova Persei* be confirmed, we shall indeed have advanced in our knowledge of what makes a new star. It would be idle to speculate when to-morrow we may learn for certain, and the present is obviously the time for reticence—almost for holding our breath. But I should like Mr. Ritchey and the others concerned to feel that we are as excited as they are. Perhaps it will encourage them to renewed exertions if I reproduce the following poem from the *Globe* newspaper of November 15:—

From America.

The Nova Persei certainly
  Has caused unbounded expectation;
One only fears that it will be
  Another case of Lick creation.

It remains only to add that we are suitably glad, on this side of the Atlantic, that the Crossley reflector is "in it." This instrument was at one time, like the reflex-zenith tube, under a cloud; and again the Americans have come to the rescue of an English instrument.

The foregoing paragraph, assigning the first place among recent sensations to photographs of the nebula round *Nova Persei* must of course be understood to refer to astronomical sensations only, otherwise our friends over the water might think we had overlooked the downfall of Tammany Hall. And this event is alluded to partly in order just to say how glad we were; and partly as an excuse for reproducing the new name found for Tammany by the *Daily Telegraph*, viz. "the absolutely unparalleled focus of political and municipal fraud and extortion."

"Unparalleled focus" is good.

A new instrument called a "Stereocomparator," and described by Dr. Max Wolf in *Astr. Nach.* No 3749, seems worthy of special attention. It has long been obvious that some rapid way of comparing two plates, with an accuracy beyond that of a mere glance, is desirable: *e. g.* for detecting stars with large proper motion or parallax. Prof. E. C. Pickering's ingenious device of taking one plate *through the film*, so that two plates can be superposed film to film, has been described several times. Arapid review of the two plates is thus rendered possible in a microscope; and pairs of corresponding images with unusual position-angle or distance are quickly detected.

In the new instrument the images on two plates are viewed as in a stereoscope—one by one eye, the other by the other; and I gather that peculiar objects are detected at a glance by a distinctly physical sensation in the eyes—due no doubt to the call for re-adjustment they necessitate.

> Wenn ein Object z. B. auf der linken Platte steht, auf der rechten aber fehlt, so empfindet der Beobachter beim Uebergehen der Stelle ein Art von Schmerz. Es ist, als wenn er einen Schlag erhielte.

This is a much more rapid process even than Prof. Pickering's. Dr. Wolf gives as an instance of detection of objects with unusual P.M. that on looking at a plate of the Pleiades he noticed the B.D. stars $3^h\,35^m{\cdot}4$, $+25^\circ\,26'$, and $3^h\,34^m{\cdot}6$, $+25^\circ\,9'$ to be unusual. The first star is No. 1820 in the Cambridge (A.G.) Catalogue, and the Editor of the *Ast. Nach.* adds a note that a comparison of Lalande, WB_2, and Cambridge shows *no P.M.* This is confirmed by reference to our photographic measures at Oxford, which show no difference from Cambridge in 10 years. And surely this just proves Dr. Wolf's point? For the general body of the Pleiades is known to be moving, and this star is distinguished from them by being stationary. I gather from Dr. Wolf that the instrument is made by Zeiss of Jena, and costs about £50; and I only wish I had £50 to spend in getting one at once.

A CORRESPONDENT, from whom I feel it an honour to have elicited a letter, kindly sends a passage which is in fine contrast to the utterance of Lord Rosebery printed last month. At her suggestion I reprint this utterance again, to facilitate the comparison:—

> But against one sublime department of science I would to beg enter a respectful warning. I do so with some apprehension, because I have distributed a prize in that department of science to-night. It is dangerous, in my judgment, to study astronomy, for astronomy kills ambition. What mind, after contemplating the eternal procession of unnumbered worlds, perhaps with their infinite generations of life, their various splendours, their history, their endless rolls of celebrity, their separate myriads of heroes, can return without a disheartening sense of the pitifulness and futility of everything on his own narrow universe?

For the preservation of the comparison utterance by Mazzini an Oxford man is responsible, viz. the late J. R. Green, author of the 'Short History of the English People.' His letters have just been edited by that master-biographer Leslie Stephen, and make a delightful volume, which I am reading slowly. Presently I hope to come upon the following passage, though I cannot identify it from the index, in which neither Mazzini nor Astronomy occur (how difficult it seems to be to make a good index!):—

> "What would you have taught in schools?" asked some one of Mazzini.
>
> "One thing at any rate in all," replied Mazzini, "and that is some knowledge of Astronomy. A man learns nothing if he hasn't learnt to wonder, and Astronomy better than any Science teaches him something of the mystery and grandeur of the Universe. Now a man who feels this will soon feel something of his own greatness and mystery, and then for the first time he is a Man."

[The reference has since been sent, p. 326, and it is interesting to note that after relating the story Green says, "I wonder whether Manchester would admire that as I admire it."]

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